

Buckling Under Pressure: An Empirical Test of the Expressive Effects of Law

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Expressive theories of law assert that law has effects on behavior beyond simple deterrence. This Note tests legal expressivism by analyzing how seatbelt use has changed in response to differing state seatbelt laws. This Note separates the effects of the laws themselves from the effects of changing enforcement levels and finds that the laws have a robust effect on seatbelt use, even controlling for convictions or citations issued. Additionally, this Note finds that a highly publicized seatbelt law in one state can affect seatbelt use in other states. These findings support an expressive function of law.

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Introduction

Economic analysis of law has historically operated under the assumption that law affects behavior solely through simple deterrence.¹ Using an “imperative theory of law” to define law as an “obligation backed by a sanction,” economists have analyzed legal sanction as though it were a market price.² Under the imperative theory, little matters to a potential misfeasor besides the chances of getting caught and the extent of the expected punishment. A punishment therefore deters negative behavior by imposing a cost on that behavior.

In the past two decades, however, legal theorists have started looking at other ways in which laws shape behavior. Some of these theorists claim that law has an “expressive” function: when a legislature passes a statute or a court hands down a decision, it communicates social values.³ In this way, law affects behavior not only by what it *does* but also by what it *says*.⁴ By expressing social values, law is able to change social norms and thereby change behavior. A law can still have a simple deterrent effect even if it also has an expressive effect. Expressive theory does not deny that sanctions have independent force; it simply adds another way that law can affect behavior. Also, the expressive function of law may contain a deterrent component—when an actor decides whether to violate a social norm, the threat of condemnation is a cost that deters bad behavior—but this cost differs from the nominal deterrent. The “market

1. See Richard H. McAdams, *A Focal Point Theory of Expressive Law*, 86 VA. L. REV. 1649, 1650 (2000).

2. Robert Cooter, *Expressive Law and Economics*, 27 J. LEGAL STUD. 585, 585 (1998).

3. *Id.* at 586.

4. Richard H. McAdams, *An Attitudinal Theory of Expressive Law*, 79 OR. L. REV. 339, 339 (2000).

price” of the illegal act is no longer simply the expected punishment; it also includes any costs that accompany norm violation.

Fifteen years ago, Cass Sunstein ended his article *On the Expressive Function of Law* with several empirical questions, including: “To what extent have shifts in norms been a function of law?”⁵ While legal scholars have been discussing the expressive functions of law since the 1990s, there has been little empirical research addressing this question, either demonstrating the existence of the expressive function of law or investigating the extent of its impact.

In what appears to be the only direct empirical investigation of these issues to date, Patricia Funk studied expressive law in a public good situation, showing that voting in Swiss Cantons declined after the repeal of mandatory voting laws that carried nominal fines.⁶ She also found that introducing postal voting did not increase turnout, despite lowering transaction costs.⁷ Funk thereby began to gauge the relative strength of the expressive function of law compared to other potentially behavior-shaping mechanisms.⁸ Her study was a first step toward drawing conclusions about law’s expressive effect, but it left many open questions. Funk noted that she used a small panel (five Cantons with staggered changes in law), which allowed her to draw only limited conclusions.⁹

Other questions in the literature on expressive law remain open to empirical examination. It is not yet apparent what aspects of a law—both in the circumstances surrounding its passage and in the content of the law itself—allow it to express values and change behavior. Specifically, empirical research can address whether fines have expressive value; more generally, studies can look at whether sanctions reinforce the expressive content of law or instead undermine existing norms.

The aim of this Note is to expand on Funk’s findings and add to the empirical literature on the expressive function of law through a case study. This note analyzes the impact of state seatbelt laws on seatbelt use in order to demonstrate the expressive aspects of these laws. Using Ordinary Least Squares (“OLS”) regressions on a large panel data set, including unique data on

5. Cass R. Sunstein, *On the Expressive Function of Law*, 144 U. PA. L. REV. 2021, 2052 (1996).

6. Patricia Funk, *Is There an Expressive Function of Law? An Empirical Analysis of Voting Laws with Symbolic Fines*, 9 AM. L. & ECON. REV. 135 (2007). Funk notes that while legal academics are confident in law’s expressive function, evidence for expressive effects has been largely anecdotal. *Id.* at 137 (“Although there is scant empirical research on expressive effects of law, legal scholars start taking them for granted.”). Robert Cooter has anecdotally observed a high level of compliance with “pooper-scooper” laws and airport smoking laws, even though enforcement of either offense is rare. While his observations indicate an expressive effect of these laws, he has not rigorously studied their enforcement or compliance level. See Cooter, *supra* note 2, at 595.

7. Funk, *supra* note 6, at 145.

8. See *id.* at 139.

9. *Id.* at 155.

state enforcement levels, the Note separates the effect of the laws themselves from the effect of enforcement. This Note not only show that low-enforcement “secondary” laws have a significant effect on use, but also find that the presence of a primary or secondary law has a robust impact on seatbelt use after controlling for actual enforcement level. It then shows that when states began to adopt seatbelt laws, seatbelt use rose substantially in states that did not have laws. Finally, the Note presents evidence that the early Tennessee seatbelt law, which did not provide for a fine, had an effect on use, despite failing to threaten sanction. This Note argues that all of these results point to an expressive effect of law.

This Note adds significantly to the prior literature in several ways. First, it responds to Funk’s call for a larger data set with more changes by using panel data on seatbelt use for fifty states, plus the District of Columbia, from 1975 to 2007. Every state started out with no law, and today, every state except for New Hampshire has a seatbelt law.¹⁰ Nineteen states and the District of Columbia switched from secondary enforcement to primary enforcement laws during the period of study.¹¹ The many changes in state laws make it possible to test the effects of these laws and the difference between the effects of primary and secondary laws. Although enforcement level data were not available for every state or every year, the data include at least some enforcement data from twenty states. This Note also adds to Funk’s article by analyzing an American data set, ensuring that any expressive function of law the study finds is applicable to the United States. Finally, this Note looks at a law with both a simple deterrent component and an expressive component—making it more representative than Funk’s voting law, which had no simple deterrent component—and attempts to separate out these two effects.

The empirical work in this Note was designed to broadly test the existence of an expressive effect, not the nuances of what makes an expressive law work; however, its results speak to several questions. Because it finds that seatbelt laws do have an expressive effect, this study weighs strongly against any theory suggesting that laws backed by a sanction, or specifically by a fine, cannot have any expressive effect. Significantly, because it finds that primary enforcement laws have a larger effect than low-enforcement secondary laws, even when controlling for enforcement, this study supports the idea that sanction itself has expressive force. In other words, a law cannot be mentally divided into the announcement of a norm, which has expressive value, and the threat of sanction, which has pure deterrent value. The threat itself works to express the norm.

10. *Safety Belt Use Laws*, INS. INST. FOR HIGHWAY SAFETY, <http://www.iihs.org/laws/SafetyBeltUse.aspx> (last visited Mar. 16, 2011).

11. Five more states switched from secondary to primary enforcement after the period of study. *Id.*

While this Note does not address normative questions, its results support the efforts of legal theorists such as Cass Sunstein and Dan Kahan, who have worked to develop consequentialist theories of expressive law. By showing that law has an expressive function outside a laboratory setting, this study indicates that legal scholars and policymakers must acknowledge expressivism in order to have a full understanding of how laws may affect behavior.

Part I, reviews the literature on expressive law, exploring how different scholars have created descriptive and normative theories from the central idea that laws make statements that can affect behavior. Part II discusses the history of seatbelt laws and previous research on the effects of laws with different enforcement levels, enforcement mechanisms, and publicity efforts. It also explains how seatbelt laws allow us to examine expressive law empirically. Part III explains the methodology. Part IV discusses the findings and argues against alternative explanations. Part V analyzes the implications of this study's findings for expressive theories. This Note then concludes and recommends further lines of study.

I. Expressive Law

Scholarship on expressive law starts with the understanding that legal actions—lawmaking, law enforcement, and court judgments—can make statements expressing values, and that these statements can affect behavior. Academics have taken this idea in two directions. Some scholars have tried to tease out how, economically or psychologically, law's expressive statements succeed in altering behavior. These scholars have also theorized about what aspects of a law give it an expressive effect.¹² Other scholars have tried to determine the normative implications of the expressive function of law, suggesting either that lawmakers should capitalize on law's expressive effects to induce desirable behavior or that lawmakers should only make decisions that express appropriate values, whether or not those decisions actually change social norms.¹³

The empirical work presented in this Note largely addresses a broad question that comes prior to the issues of how the expressive function works or

12. See, e.g., Cooter, *supra* note 2 (developing an economic theory of expressive law and positing two mechanisms by which law can change social norms); Yuval Feldman & Janice Nadler, *The Law and Norms of File Sharing*, 43 SAN DIEGO L. REV. 577 (2006) (examining which characteristics of law affect social norms by surveying students' perceptions of file sharing laws); McAdams, *supra* note 1 (suggesting law provides a "focal point" around which people coordinate action); McAdams, *supra* note 4 (theorizing that law changes behavior by signaling community values).

13. See, e.g., Matthew D. Adler, *Expressive Theories of Law: A Skeptical Overview*, 148 U. PA. L. REV. 1363 (2000) (arguing that normative expressive theories of law are not persuasive); Elizabeth S. Anderson & Richard H. Pildes, *Expressive Theories of Law: A General Restatement*, 148 U. PA. L. REV. 1503 (2000) (arguing that a central point of constitutional law is and should be to express appropriate attitudes through state action); Sunstein, *supra* note 5 (noting that norm management is legitimate if it achieves desirable ends).

how lawmakers should use law's expressive effect. It asks: does law actually have an observable expressive effect? Can we measure the norm-shaping power of a law? However, because the results of this study speak to questions posed in the literature, and because understanding the scope of expressive theory will highlight the importance of empirically validating law's expressive function, this Part reviews the academic literature on expressive law.

A. *Descriptive Theories of Expressive Law*

Expressive theorists have described a variety of mechanisms through which laws work to shape social norms and behavior. Some theorists posit that law changes behavior by letting people know how society will react to their actions, which encourages them to act in ways society endorses. Others argue that law influences behavior by changing social norms directly. And still others suggest that law changes individual values and thereby shapes social norms. These scholars do not tend to present their proposed mechanisms as mutually exclusive alternatives.¹⁴ Several, or even all, of these mechanisms may explain law's expressive effects.

Several theorists have posited that expressive laws influence behavior by indicating to individuals how society will understand and respond to their actions. Of these theorists, some suggest that the law *creates* the anticipated reaction, while others suggest the law signals that a norm already exists. Cass Sunstein and Lawrence Lessig, in the first camp, have proposed a "meaning account" of the expressive function of law.¹⁵ They argue that laws can change the social meaning of an action: an action with a bad meaning may bring social condemnation, which makes the action more costly, whereas an action with a good meaning may have positive social effects. Lessig has used the example of two historical laws designed to eliminate dueling to explore how law can change meaning. One law simply prohibited dueling. This left the social meaning of dueling unchanged: a man duels because he has a duty to defend his honor. The prohibition only added a cost to dueling—potential jail time—that the potential dueler might have weighed against his duty. The other law made a man ineligible for public office if he participated in a duel. Under this regime, a man who refused to duel risked shirking his duty, but he might also have embraced a competing social duty: the duty to serve one's community. The law, therefore, ambiguated the social meaning of dueling.¹⁶ Sunstein has noted

14. See, e.g., Dhammika Dharmapala & Richard H. McAdams, *The Condorcet Jury Theorem and the Expressive Function of Law: A Theory of Informative Law*, 5 AM. L. & ECON. REV. 1, 2 (2003) (noting that the authors' article presents an "additional explanation" to Cooter's and McAdams's theories).

15. See Lawrence Lessig, *Social Meaning and Social Norms*, 144 U. PA. L. REV. 2181, 2185 (1996); Sunstein, *supra* note 5, at 2032-33.

16. See Lessig, *supra* note 15, at 2186-87.

that a meaning account would explain an expressive effect of seatbelt laws and laws governing other risk-taking behaviors.¹⁷

Legal economist Richard McAdams falls into the second camp, suggesting that laws signal existing norms. McAdams has put forward an attitudinal theory of expressive law that, like the “meaning account,” acknowledges the importance of society’s opinion of an act. Unlike Sunstein and Lessig, who discuss laws that actively change social meaning, McAdams argues that “law changes behavior by *signaling* the underlying attitudes of a community or society.”¹⁸ McAdams claims that individuals are both intrinsically and extrinsically motivated to gain the approval of others, including strangers,¹⁹ but they have imperfect information about what others will endorse. Because “democratically produced legislative outcomes are positively correlated with popular attitudes,” individuals use laws as signals of societal values.²⁰ Even if legislation is not as a whole correlated with popular opinion, McAdams argues, law can still have an expressive effect if there are certain categories of legislation—such as well-publicized laws and those that are made without the input of interest groups—that can be easily recognized as representative of public opinion.²¹

Cooter has argued that law has two expressive uses: first, it can change social norms directly by solving collective action problems; and second, it can change social norms by shaping individual values.²² In the first instance, Cooter envisions a world in stable social equilibrium—rightdoers and wrongdoers receive the same payoff for their behavior. Even if this world contains a Pareto-superior equilibrium with more rightdoers, it is against the interest of any individual wrongdoer to begin complying, so the system cannot jump to this preferred equilibrium. The enactment of a law can solve this collective action problem by creating a new focal point. If enough people obey the law out of respect, the system shifts into the superior equilibrium.²³ In this way, the law creates a social norm without changing individual values. Individuals obey out of respect for the law, but by changing their behavior, they change what is

17. Sunstein, *supra* note 5, at 2052.

18. McAdams, *supra* note 4, at 340 (emphasis added).

19. In one experiment, researchers had subjects play the ultimatum game with a twist. In earlier iterations, players had been anonymous to each other, but the researchers could observe which player gave which offer. In this study, however, subjects were told that the experimenters would also be blind to their bids. Under these circumstances, offers dropped considerably, indicating that some of the generous behavior observed in ultimatum games may be motivated by a desire for the researchers’ approval. *Id.* at 344-45 (discussing Elizabeth Hoffman et al., *Preferences, Property Rights, and Anonymity in Bargaining Games*, 7 GAMES & ECON. BEHAV. 346 (1994)).

20. McAdams, *supra* note 4, at 340.

21. *Id.* at 362.

22. See Cooter, *supra* note 2, at 586.

23. *Id.* at 594.

socially expected.²⁴ Also, the law may create a social norm without signaling that the norm existed before the law. This first mechanism speaks largely to public goods laws, such as regulations prohibiting littering, where, in a world of compliance, it is ordinarily in the individual's interest to defect. It does not directly apply to laws restricting risky behavior, such as seatbelt laws.

Discussing his second theory, Cooter posits that a rational person will want to change her preferences when the opportunities presented by this observable change in character are superior according to both new preferences and old preferences.²⁵ Imagine, for instance, a person who values both income and leisure. If she changes this preference for leisure to a preference for hard work, she may, as a result, receive so much income that her life is superior, even under her old set of preferences.²⁶ Cooter calls this change "Pareto self-improvement" and notes that a law will prompt improvements in character whenever sanctions create an opportunity for Pareto self-improvement.²⁷ In this way, law changes internalized values. Collectively, changed individual values may create a new social norm.

Cooter describes how these two expressive uses of law complement the legal function of deterrence. He likens social norms to imperative laws, defining a social norm as "an obligation backed by a social sanction."²⁸ Obeying a norm—one created by the law's expressive power or otherwise—can produce social benefits such as praise and esteem that may offset the costs of obeying. Violating a norm, on the other hand, imposes heavy social costs.²⁹ The state can supplement these social costs with legal sanctions, increasing the overall cost of disobeying the norm through simple deterrence.³⁰ Additionally, if a person internalizes a norm by changing her preferences, she experiences a personal cost for violating the norm and is therefore willing to pay to observe it.³¹ Through expressivism, deterrence, and internalization, a law could change a person's preferences and thereby change her choices.

The meaning account, McAdams's attitudinal theory, and Cooter's two economic theories all explicitly rely on the expressive power of law to explain behavioral changes. However, law may also affect behavior in ways that fit neither the simple deterrence theory (the legal sanction itself acts as a cost) nor the usual expressivist theory (the law changes behavior by expressing values and changing social norms). A law may have a "long-term deterrence" effect,

24. *Id.* at 595.

25. *Id.* at 600.

26. *Id.*

27. *Id.* at 605.

28. Robert D. Cooter, *Three Effects of Social Norms on Law: Expression, Deterrence, and Internalization*, 79 OR. L. REV. 1, 5 (2000).

29. *Id.* at 7-8.

30. *Id.* at 15.

31. *Id.* at 7.

where an immediate threat over long periods of time leads to habit formation and moral education, which eventually overtake simple deterrence as the main reason for obedience.³² Even if the law does not change people's values directly, a long period of obedience leads people to settle into a routine—like buckling up when they enter a car—that makes obedience easy. A safety law can also make it psychologically easier for a person to take a precaution. A voluntary action may force someone to confront her own mortality; but if she believes she is taking a precaution only because the law says she must, she can avoid the discomfort of pondering death.³³ As McAdams and Dhammika Dharmapala argue, the law may not express social norms *per se*, but rather the legislature's superior information about an activity's risks.³⁴ And, as Cooter suggests, people may obey the law out of sheer civic duty or respect.³⁵

B. *Maximizing the Expressive Function of Law*

In addition to debating how the expressive mechanism works, scholars have hypothesized about what qualities of a law maximize its expressive power. Some of these ideas concern the circumstances surrounding the law and its passage, in contrast to the law itself. McAdams has suggested that well-publicized laws and laws passed without the influence of interest groups are most likely to have an expressive effect because they are most easily recognized as representative of public opinion.³⁶

Other ideas address features of laws themselves, such as the expressive value of fines. Several scholars have hypothesized that deterrence mechanisms, including fines, have "positive effects on the internalization of existing norms" because the threat of sanction is sufficient to convince people that an act is morally problematic.³⁷ In this view, any sanction, including a fine, has a significant expressive effect. Others, however, have claimed that an external motivation, such as a fine, can crowd out internal motivations and therefore have a destructive effect on norms. By putting a price on misbehavior, these theorists suggest, lawmakers communicate that citizens are entitled to commit an immoral act if they pay the fine, and individuals internalize this message.³⁸

32. See H. LAURENCE ROSS, *DETERRING THE DRINKING DRIVER* 8 (1984).

33. John J. Donohue III, *Using Market Incentives To Promote Auto Occupant Safety*, 7 *YALE L. & POL'Y REV.* 449, 454 (1989).

34. See Dharmapala & McAdams, *supra* note 14.

35. See Cooter, *supra* note 2, at 594.

36. See McAdams, *supra* note 4, at 362.

37. See Feldman & Nadler, *supra* note 12, at 595. Other scholars, while not denying that fines have an expressive effect, argue, at least partially from survey data, that the condemnation expressed by a fine is substantially lower than that expressed by imprisonment or some alternative sanctions. See Dan M. Kahan, *What Do Alternative Sanctions Mean?*, 63 *U. CHI. L. REV.* 591, 593, 623 (1996).

38. See Feldman & Nadler, *supra* note 12, at 591-93. For example, a well-known case study involves a daycare center that tried to solve the problem of parents arriving late to pick up their children

There are even a few authors who argue that law has almost no effect on social norms, because existing norms are robust, and few people know the specifics of criminal law. According to these scholars, norms influence people's perception of the law, not the other way around.³⁹

C. *Normative Theories of Expressive Law*

Another branch of scholarship on expressive law starts with the premise that legal decisions express society's values, and then uses that premise to develop normative theories of how lawmakers should act. Sunstein has differentiated between two normative theories of expressive law, corresponding to two ways of understanding law's expressive function.⁴⁰ One theory is consequentialist: when a law makes a statement, that statement affects social norms, which in turn shape behavior. Lawmakers should therefore favor laws that have a desirable effect on behavior.⁴¹ The second theory concerns integrity: lawmakers should favor laws that make statements with desirable social meanings, even if the laws do not actually shape social norms.⁴² The integrity theory is indifferent to the consequences of legal action—indifferent to whether law has expressive effects—so a study addressing whether the effects exist does not contribute to this debate.

A number of articles have addressed expressivism as a consequentialist normative theory. Sunstein himself asserts that support for a legal "statement" should be rooted in "plausible judgments about its effect on social norms and hence in 'on balance' judgments about its consequences."⁴³ Some scholars have said that this sort of consequentialism is properly understood as part of deterrence theory, not expressivism. Kahan writes: "Whereas expressive theory focuses on meanings, deterrence theory focuses on consequences. According to deterrence theory, society should punish if, and to the extent that, doing so maximizes social welfare."⁴⁴ Terminology aside, Sunstein and Kahan agree that the expressive function of law can shape behavior, and we should use this power to achieve desirable ends. Kahan, for example, has suggested that policymakers can solve collective action problems by shaping social norms so that individuals perceive that most members of their community are contributing to a collective good, thereby generating trust within the

by instituting a late fee. Once the fine was in place, many more parents arrived late. See Uri Gneezy & Aldo Rustichini, *A Fine Is a Price*, 29 J. LEGAL STUD. 1 (2000).

39. See Feldman & Nadler, *supra* note 12, at 594 (discussing John M. Darley, Kevin M. Carlsmith & Paul H. Robinson, *The Ex Ante Function of the Criminal Law*, 35 LAW & SOC'Y REV. 165 (2001)).

40. See Sunstein, *supra* note 5, at 2025.

41. *Id.* at 2025-26.

42. *Id.* at 2026-27.

43. *Id.* at 2045.

44. Dan M. Kahan, *The Secret Ambition of Deterrence*, 113 HARV. L. REV. 413, 425 (1999).

community. Individuals, he argues, will then act as reciprocators and contribute to the good.⁴⁵

Arguing about how we should capitalize on law's expressive effects presupposes that these effects are real and observable. Empirical evidence of an expressive function of law supports this presupposition and thereby supports the normative mission of these scholars.

II. Seatbelt Laws

The National Safety Council estimates that one out of every eighty-eight Americans will die in a motor vehicle accident, and one-third of the victims will be car occupants at the time of death.⁴⁶ Fortunately, when lap/shoulder seatbelts are used properly, they reduce the risk of fatal injury to front-seat occupants by forty-five percent.⁴⁷ Because seatbelt use is the single most important factor in preventing or reducing serious injury to occupants in car accidents, increased seatbelt use has been a major goal of government agencies and insurance companies for more than thirty years.⁴⁸ This Part discusses the history of these laws and explains why statistical analysis of these laws and their effects permits broader conclusions about the expressive function of law.

A. History

In 1961, New York passed legislation mandating that seatbelts be installed in every vehicle sold in the state,⁴⁹ and front-seat driver and passenger belts became standard issue in all 1964 model year automobiles.⁵⁰ The National Traffic and Motor Vehicle Safety Act of 1966 created the National Highway Traffic Safety Administration ("NHTSA"), and in 1967, NHTSA started requiring seatbelts in all cars.⁵¹ A battle between the automotive industry and the insurance industry over mandatory passive restraints (specifically, airbags and automatic seatbelt systems) culminated in 1984, when NHTSA announced

45. See Dan M. Kahan, *Reciprocity, Collective Action, and Community Policing*, 90 CALIF. L. REV. 1513 (2002) (arguing that policies promoting trust can solve collective action problems and that a system of selective privatization best accomplishes this goal in the case of community policing); see also Kahan, *supra* note 37, at 635 (arguing that shaming sanctions are a viable alternative to imprisonment because they deter as successfully and adequately express society's condemnation of the criminal). But see Dan M. Kahan, *What's Really Wrong with Shaming Sanctions*, 84 TEX. L. REV. 2075 (2006) (acknowledging that shaming sanctions are not viable because they express values rejected by certain groups of citizens).

46. NAT'L SAFETY COUNCIL, INJURY FACTS 37 (2011), available at http://www.nsc.org/NSC%20Picture%20Library/News/web_graphics/Injury_Facts_37.pdf.

47. JAMES HEDLUND ET AL., NAT'L HIGHWAY TRAFFIC SAFETY ADMIN., HOW STATES ACHIEVE HIGH SEATBELT USE RATES 1 (2008).

48. *Id.*

49. Donohue, *supra* note 33, at 459.

50. HEDLUND ET AL., *supra* note 47, at 3.

51. Donohue, *supra* note 33, at 460.

that automatic restraints would be required in all new cars starting with model year 1990, unless enough states passed mandatory seatbelt laws to cover two-thirds of the U.S. population.⁵²

In response to this rule, New York enacted the nation's first mandatory seatbelt law.⁵³ The law was highly publicized, appearing on the front page of the *New York Times* twice in 1984.⁵⁴ New York's law had a primary enforcement mechanism, meaning police officers could pull over a driver for not wearing a seatbelt and fine her for the offense. When New Jersey passed the country's second law just months later, however, it contained only a secondary enforcement mechanism: a police officer could only ticket a driver for failure to wear a seatbelt if the officer pulled her over for another violation.⁵⁵ Other states enacted their own laws, and most followed New Jersey's lead, adopting secondary enforcement.⁵⁶ By 1990, thirty-four states had enacted seatbelt laws.⁵⁷ In 1993, California became the first state to upgrade from a secondary law to a primary law.⁵⁸ Since then, twenty-four states plus the District of Columbia have upgraded.⁵⁹ Congress announced that to be eligible for millions of dollars in federal transportation grants for fiscal year 2010, states had to enact primary seatbelt laws by June 30, 2009.⁶⁰ Florida, Minnesota, and Wisconsin responded by passing primary law upgrades that went into effect in June 2009.⁶¹ Today, every state except for New Hampshire has a mandatory seatbelt law.⁶²

52. *Id.* at 461-62.

53. *Id.* at 462; see N.Y. VEH. & TRAF. LAW § 1229-c (McKinney 1996).

54. Esther B. Fein, *State To Enforce Seat-Belt Law, Starting Tonight*, N.Y. TIMES, Dec. 31, 1984, at A1; Michael Oreskes, *Leaders in Albany Agree To Require Use of Seatbelts*, N.Y. TIMES, June 20, 1984, at A1.

55. Charles M. Farmer & Allan F. Williams, *Effect on Fatality Risk of Changing from Secondary to Primary Seatbelt Enforcement*, 36 J. SAFETY RES. 189, 189 (2005); see N.J. STAT. ANN. § 39:3-76.2f (West 2009).

56. See INS. INST. FOR HIGHWAY SAFETY, *supra* note 10.

57. *Id.*

58. *Id.*

59. *Id.*

60. See 23 U.S.C. § 405(b) (2006).

61. Larry Copeland, *Money Steers Seatbelt Debate*, USA TODAY, May 14, 2009, at 3A; Press Release, PR Newswire, Minnesota Enacts Lifesaving Seatbelt Law (May 22, 2009), available at <http://in.sys-con.com/node/974355>; Press Release, PR Newswire, On Wisconsin! Badger State Becomes 30th To Enact Critical Seatbelt Law (June 30, 2009), available at <http://www.reuters.com/article/pressRelease/idUS171626+30-Jun-2009+PRN20090630>.

62. The New Hampshire House passed a primary enforcement bill early in 2009, but the Senate first amended it to allow only for secondary enforcement and then voted to table the bill indefinitely. *Bills Tabled on Death Penalty, Seatbelts*, UNION LEADER (Manchester, N.H.), Apr. 30, 2009, at A9. New Hampshire does have a primary enforcement law requiring children under eighteen to wear seatbelts. *Seatbelt Laws*, GOVERNORS HIGHWAY SAFETY ASS'N, http://www.ghsa.org/html/stateinfo/laws/seatbelt_laws.html (last visited Feb. 5, 2011).

B. Ripeness for Empirical Study of Expressive Law

Seatbelt laws provide a useful mechanism for empirical study both because they contain some of the hallmarks of expressive laws and because of the high-quality data on seatbelt use, seatbelt laws, and law enforcement.

It is reasonable to expect that seatbelt laws would have a moderate or strong expressive effect. First, under McAdams's attitudinal theory, local laws should have a greater expressive effect than national laws, because attitudes of approval and disapproval are local.⁶³ Seatbelt laws, with their varying fines, dates of passage, and enforcement mechanisms are unique to each state and therefore may express a state's character. Notably, citizens of New Hampshire take pride in their lack of a seatbelt law, and New Hampshire newspapers have noted that a law would change the state's character.⁶⁴ Second, McAdams also hypothesizes that laws perceived to be legitimate, or passed without the influence of special interest groups, will have a greater expressive effect, because the connection between public attitudes and the law will be more apparent.⁶⁵ While the insurance industry has been a driving force in passing seatbelt legislation,⁶⁶ insurance companies have an interest in reducing injuries, so their support for a precaution may be a good indicator of its value. Automakers may have approved of early seatbelt laws largely because they hoped they would prevent an airbag requirement, but a majority of New Yorkers favored a mandatory seatbelt law when the bill was passed, revealing that the legislature enacted the law in line with perceivable public attitudes.⁶⁷ Finally, anecdotal experience indicates that failure to comply with reasonable personal safety protocols—by, say, smoking, having unprotected sex with strangers, or walking alone at night in a dangerous area—can draw heavy social sanctions, even though the transgressor does not necessarily impose significant public costs. When a law expresses that seatbelts are a reasonable safety protocol, we can expect that social sanctions will supplement legal sanctions and deter noncompliance.

Both the quality of data on seatbelt use and enforcement and the staggered, state-by-state enactment of seatbelt laws allow for a statistically powerful study of expressive effects. While pooper-scooper laws, for example, might have strong expressive effects, state and city laws may overlap, making it

63. McAdams, *supra* note 4, at 341.

64. See, e.g., Editorial, *State Protections: Seatbelts First, Then What?*, UNION LEADER (Manchester, N.H.), Apr. 19, 2009, at B2 ("New Hampshire is a place where individuals have always been protected from the overreaching of the majority. Once the seat-belt bill becomes law, that tradition cherished by all of us will end.").

65. McAdams, *supra* note 4, at 341.

66. JAMES L. NICHOLS & KATHERINE A. LEDINGHAM, NAT'L COOP. HIGHWAY RESEARCH PROGRAM, THE IMPACT OF LEGISLATION, ENFORCEMENT, AND SANCTIONS ON SAFETY BELT USE 6 (2008), available at http://onlinepubs.trb.org/onlinepubs/nchrp/nchrp_rpt_601.pdf.

67. Oreskes, *supra* note 54.

hard to separate out the effect of a single law.⁶⁸ Seatbelt laws, on the other hand, are uniform within states. It is therefore difficult to measure how much pooper-scooper laws change behavior, whereas it is possible to measure statewide levels of seatbelt use, either through surveys or analysis of fatal car accidents (NHTSA has collected data using both of these metrics).⁶⁹ Additionally, a number of states have collected data on the annual number of tickets or convictions,⁷⁰ so it is possible to study how enforcement affects use. Finally, had the laws all been passed in the same year, it might be difficult to tell whether the laws or other unrelated events had changed behavior. But because different states passed laws at different times, it is possible to safely attribute shifts that regularly occur after enactment to the laws themselves.

C. *Past Study of Seatbelt Laws*

A substantial body of empirical literature has developed around seatbelt laws, with many studies investigating the impact of seatbelt laws on seatbelt use. However, only one 1988 study has completely separated the effect of the enforcement mechanism from the effect of enforcement level.⁷¹ This study, conducted by B.J. Campbell, then Director of the University of North Carolina Highway Safety Research Center, used mail survey responses from twenty states disclosing how many citations they issued per year. He controlled for population to find that use correlated with enforcement level, and, for a given level of enforcement, states with primary laws had higher seatbelt use than states with secondary laws.⁷²

Other studies have looked at the impact of primary laws and secondary laws without controlling for enforcement. This scholarship have consistently found that states with primary laws have a higher rate of seatbelt use than states with secondary laws, which in turn have higher use than states without any law at all, and these higher use rates correspond to fewer fatalities.⁷³ They also note

68. See N.Y. CITY DEP'T OF HEALTH AND HYGIENE BD. OF HEALTH, NOTICE OF ADOPTION OF AMENDMENTS TO SECTIONS 161.03, 161.04, AND 161.05 OF THE NEW YORK CITY HEALTH CODE (2004), available at <http://www.nyc.gov/html/doh/downloads/pdf/public/161-03-notice.pdf>.

69. See *infra* Part IV.

70. See *infra* Appendix A.

71. B.J. Campbell, *The Association Between Enforcement and Seatbelt Use*, 19 J. SAFETY RES. 159 (1988).

72. *Id.* at 161.

73. See, e.g., Alma Cohen & Liran Einav, *The Effects of Mandatory Seatbelt Laws on Driving Behavior and Traffic Fatalities*, 85 REV. ECON. & STAT. 828 (2003); Farmer & Williams, *supra* note 55; Frederick P. Rivara, Diane C. Thompson & Peter Cummings, *Effectiveness of Primary and Secondary Enforced Seatbelt Laws*, 16 AM. J. PREVENTIVE MED. 30 (Supp. I 1999); Ruth A. Shults et al., *Effectiveness of Primary Enforcement Safety Belt Laws and Enhanced Enforcement of Safety Belt Laws: A Summary of the Guide to Community Preventive Services Systematic Reviews*, 35 J. SAFETY RES. 189 (2004).

that a switch from a secondary law to a primary law significantly increases use.⁷⁴

Instead of focusing on overall enforcement level, some of these studies have closely examined specific enforcement programs, such as Click It or Ticket (“CIOT”) campaigns, which consist of short-duration blitzes of high enforcement accompanied by advertising campaigns.⁷⁵ These studies have concluded that high-visibility enforcement, especially through checkpoints and roadblocks, is associated with increased seatbelt use,⁷⁶ and some experts have stated that high perception of enforcement, independent of actual enforcement, is the single most important element of an enforcement policy.⁷⁷ These studies have also noted a correlation between the number of citations issued during a blitz period and seatbelt use.⁷⁸ The literature is mixed on the impact of paid media campaigns, with some studies finding that paid advertisements are associated with increases in use and others finding little to no correlation between media dollars spent and level of seatbelt use.⁷⁹ Survey data have shown a correlation between seatbelt use and perception of how likely a person is to be ticketed if he does not wear his seatbelt for six months, with 48% of those in high-use states saying it would be “very likely” that the violator would receive a ticket, in contrast to 32% of respondents in medium-use states and low-use states.⁸⁰ Existing studies of seatbelt laws have been remarkably thorough in approach, combining broad national analysis with in-depth case studies to determine how best to maximize seatbelt use through law.

III. Motivations and Methods

A. *Creating the Dependent Variable*

This Note looks at the effects of seatbelt laws and their enforcement on seatbelt use. Therefore, the left-hand side (dependent) variable in almost every regression in this Note is “Use,” expressed as the percentage of vehicle occupants who wear their seatbelts. I obtained data on seatbelt use through two channels. First, I obtained survey data collected by the individual states and

74. See Farmer & Williams, *supra* note 55.

75. See HEDLUND ET AL., *supra* note 47.

76. See *id.*; NICHOLS & LEDINGHAM, *supra* note 66.

77. Telephone Interview with Michele Fields, Gen. Counsel, Ins. Inst. for Highway Safety (May 1, 2009).

78. See HEDLUND ET AL., *supra* note 47, at iii.

79. Compare NICHOLS & LEDINGHAM, *supra* note 66, at 2 (“[P]ublicity was essential for program impact, with paid ads associated with greater increases than other forms of media activity, but with earned media . . . also essential for maximum impact.”), with HEDLUND ET AL., *supra* note 47, at 17 (“While the differences are not significant, it’s notable that low belt use States spent more media dollars per capita, total and for television and radio separately, than high belt use States in each of the 2003 and 2004 [Click It or Ticket] efforts.”).

80. HEDLUND ET AL., *supra* note 47, at 21 tbl.13.

NHTSA. For the period from 2001 to 2007, data come from NHTSA's State and Territory surveys.⁸¹ Liran Einav and Alma Cohen generously shared survey data they had gathered for the period from 1983 to 1997. The Einav and Cohen data come from both NHTSA and phone calls to the Highway Safety Offices of each state.⁸² Second, I obtained data from the Fatality Analysis Reporting System ("FARS"), a data system created by NHTSA and designed by the National Center for Statistics and Analysis.⁸³ FARS contracts with an agency in each state to collect data on every motor vehicle crash on a public road that involves a fatality within thirty days of the crash. The system has been in place since 1975. This study uses FARS data classifying passenger occupant fatalities in each state and year by restraint use.⁸⁴ Inevitably, in most states and years, the restraint use of some fatally injured occupants was not known. This Note therefore uses the "Percent 'Known' Restrained" as its basis for actual seatbelt use.

Each of these two data sets has problems and virtues. The survey use data only goes back to 1983, and data are unavailable for a significant number of state-year combinations. However, the survey data most likely represent a close approximation of actual use, as confirmed by the robustness to different measures. A close approximation to actual use, as opposed to a mere correlation with use, allows linear regressions to reveal the actual, absolute impact of a policy, not simply whether it has an effect. If a right-hand side variable signifies the law's expressive effect,⁸⁵ the coefficient on the variable indicates the expressive power of that law. The FARS data, on the other hand, do not, in an absolute sense, closely approximate actual use. Clearly, FARS data will vastly understate actual use because people are less likely to die if they are restrained. In addition, reckless drivers—those more likely to get into serious accidents—are less likely to wear their seatbelts than safe drivers.⁸⁶ The great advantage of the FARS data, however, is that they cover every state plus the District of Columbia for every year from 1975 to 2007.

81. *Seatbelt Use in 2007—Use Rates in the States and Territories*, NAT'L HIGHWAY TRAFFIC SAFETY ADMIN., <http://www-nrd.nhtsa.dot.gov/Pubs/810949.pdf> (last visited Apr. 21, 2011).

82. Cohen & Einav, *supra* note 73, at 831. The authors found that the data from these two sources matched closely and that both were in line with data from the CDC's Behavioral Risk Factor Surveillance System, a state level telephone survey, and these similarities verified the robustness of their data. *Id.* at 832.

83. *State Data Program*, NAT'L HIGHWAY TRAFFIC SAFETY ADMIN., <http://www-fars.nhtsa.dot.gov/Main/index.aspx> (follow the FARS hyperlink at the bottom of the page) (last visited Apr. 21, 2011).

84. This data comes from a report generated by the National Center for Statistics and Analysis's Information Services Team (on file with author).

85. A variable could signify the law's expressive effect by being an indicator variable for a policy, where other variables in the regression account for all other ways in which the policy affects behavior.

86. HEDLUND ET AL., *supra* note 47, at 9.

To capitalize on the best attributes of both data sets—the accuracy of the survey data and the large number of observations in the FARS data—I ran a simple OLS regression with “Survey Use” as the dependent variable and “FARS Percent ‘Known’ Restrained” as the independent variable:

$$\text{Survey_Use} = \alpha(\text{FARS}) + \beta$$

Here, $\alpha = 1.302$ ($\sigma = 0.025$) and $\beta = 0.212$ ($\sigma = 0.008$). The R-squared value for this regression was 0.767, meaning over three-quarters of the variance in the survey data can be explained with the FARS data. It has been noted that while FARS data and observed use are highly correlated, some states rank higher on one measure than the other.⁸⁷ Because I do not see a reason to suspect a relationship between the level of bias in the FARS data and enforcement, I believe the high R-squared value and the additional power of so many observations together justify relying on the FARS data. Using the FARS data in conjunction with the values derived for α and β , I generated an estimate of seatbelt use for each state-year combination.⁸⁸ Unless otherwise noted, for all regressions in this Note, the dependent variable is this “Use,” a linear transformation of the Percent “Known” Restrained column in the FARS data.

B. *Investigating Expressive Effects*

I investigate four potential manifestations of the expressive effect of seatbelt laws. First, I examine both the overall impact of primary and secondary laws and the impact of these laws controlling for enforcement using OLS regressions. The first set of regressions, looking at the relative efficacy of primary and secondary laws independent of actual enforcement, was designed to test prior research on seatbelt laws. The second set of regressions—those that control for enforcement and therefore separate the expressive effect of these laws from their deterrent effect—form the heart of this Note, as they most explicitly test whether the law has an expressive function. I also look at the effect of the New York law to see whether a highly publicized law in one state can influence behavior in other states. Finally, I explore the effects of an early Tennessee law without a fine—a purely expressive law.

87. *Id.* at 11; see also NICHOLS & LEDINGHAM, *supra* note 66, at 68 app. f.

88. Acknowledging that this regression does not give a perfect estimate of use, I allow use rates to go above 100%. This happens in only fifteen state-year combinations, and use never rises above 110%.

1. The Impact of Primary and Secondary Laws

I use two different models to investigate the overall impact of primary and secondary laws: a simple indicator model and a spline/dummy hybrid model. The dummy model contains indicator variables for the presence of a primary law, the presence of a secondary law, and whether the New York law had gone into effect. Each of these variables takes the value of 1 when the condition is met—"Primary Law" is 1 in a state-year combination that has a primary enforcement law—and 0 when it is not met. I run the model five times under different specifications, including state and year fixed effects in four of the five specifications,⁸⁹ state linear trends in two specifications,⁹⁰ and a matrix of social controls in two specifications. This matrix of social controls includes per capita ethanol consumption,⁹¹ personal income per capita,⁹² percent of the population between ten and nineteen years old,⁹³ state population density,⁹⁴ and the percent of drivers from out of state.⁹⁵ The results of these regressions are included in Appendix B as Table 1. The coefficient on an indicator variable shows the percentage point increase in seatbelt use corresponding to the

89. When I control state or year fixed effects, Stata calculates a coefficient for a dummy variable for each state across all years or for each year across all states, respectively. This way, if certain states have particularly high or low use, or if several years are associated with inexplicably high or low use, the regression will account for these anomalies; it will not spuriously associate them with a law. See, e.g., Karl E. Case, John M. Quigley & Robert J. Shiller, *Comparing Wealth Effects: The Stock Market Versus the Housing Market*, 5 ADVANCES IN MACROECON. 1, 9 (2005) (using country-specific fixed effects in an analysis of the relationship between housing wealth and household consumption); Donald E. Heller, *The Effects of Tuition and State Financial Aid on Public College Enrollment*, 23 REV. HIGHER ED. 65, 72 (1999) (including a state-fixed effects model in an analysis of college enrollment data); Jith Jayaratne & Philip E. Strahan, *The Finance-Growth Nexus: Evidence from Bank Branch Deregulation*, 111 Q.J. ECON. 639, 649 (1996) (using state fixed effects in a study concluding that banking reform causes financial growth).

90. When I control for state linear trends, Stata assigns a coefficient to each state that gets multiplied by a "trend" variable. This accounts for the linear increase in use in each state—if certain states have especially rapid or slow growth in use for reasons unrelated to the law, growth will not be attributed to the law. See, e.g., Louis S. Jacobson, Robert J. LaLonde & Daniel G. Sullivan, *Earnings Losses of Displaced Workers*, 83 AM. ECON. REV. 685, 694 n.20 (1993) (discussing the use of worker-specific time trends in an analysis of long-term earning losses of displaced workers).

91. See *Per Capita Ethanol Consumption for States, Census Regions, and the United States, 1970–2007*, NAT'L INST. ON ALCOHOL ABUSE & ALCOHOLISM, <http://www.niaaa.nih.gov/Resources/DatabaseResources/QuickFacts/AlcoholSales/consum03.htm> (last visited Mar. 16, 2011). Research has suggested a strong inverse relationship between alcohol consumption and seatbelt use. NICHOLS & LEDINGHAM, *supra* note 66, at 49.

92. See *Local Area Personal Income, 2009*, BUREAU OF ECON. ANALYSIS, http://www.bea.gov/newsreleases/regional/lapi/lapi_newsrelease.htm (last visited Apr. 21, 2011). Studies have indicated that seatbelt use is correlated with socioeconomic status, HEDLUND ET AL., *supra* note 47, at 4, and income per capita is a significant predictor of whether a state will adopt a seatbelt law, Donohue, *supra* note 33, at 462.

93. Abhay Aneja, research assistant for John Donohue, generously provided this data.

94. *Id.*

95. FARS report generated by the Information Services Branch of the National Center for Statistics and Analysis (on file with author).

presence of that variable. The coefficients for both primary law and secondary laws are significant,⁹⁶ indicating that these laws have an effect.

To confirm the results of my dummy model and investigate the effect of switching from a secondary enforcement law to a primary enforcement law, I use a flexible statistical model to examine the switch from no law to a primary law, from no law to a secondary law, and from a secondary law to a primary law. This analysis requires a model that can capture how a dependent variable (seatbelt use) changes after a specific event (the passage or upgrade of the law). If use was already increasing before the new law, the law could both have an instantaneous effect on use and change the rate at which seatbelt use was increasing. The flexible model I use captures both a jump or drop in the dependent variable at the time of the event and any instantaneous acceleration or deceleration—a change in the rate of change—at the time of the event. The model looks at use before and after law passage and fits a linear function of time in each of these two regions. Because I combine a “spline model,” which captures only a change in slope,⁹⁷ with a “dummy-variable model,” which captures only a jump or drop, I refer to my model as a “spline/dummy hybrid.”⁹⁸

The hybrid model is a function of time relative to the year that the law was passed, so year “zero” is a different calendar year for each state. This can pose a problem if only one state has data for a specific year (often an extreme, either long before the law was passed or long after). If that state has an atypical use rate for that year, its data can skew the entire model. I preempt this problem by using the same states for every year in all three hybrid models: the model showing switches from no law to a primary law contains eight states and runs from nine years before the law passed to sixteen years after; the model showing switches from no law to a secondary law contains forty-one states and runs from eleven years before the law passed to six years after; and the model showing switches from a secondary law to a primary law contains seven

96. In all my regressions, I cluster standard errors at both the state and year level using *cgmreg*, a regression code developed by Colin Cameron, Jonah Gelbach, and Douglas Miller. See A. Colin Cameron, Jonah B. Gelbach & Douglas L. Miller, *Robust Inference with Multi-Way Clustering*, (U.C. Davis Dep’t of Econ., Working Paper No. 09-9, 2009), available at http://www.econ.ucdavis.edu/working_papers/09-9.pdf.

97. A graph of a spline model would show a single straight line over time with a sharp “kink” the year the law passed.

98. Ian Ayres and John Donohue have used a similar hybrid model to capture how crime levels changed after the passage of a right-to-carry law. Ian Ayres & John J. Donohue III, *Shooting Down the “More Guns, Less Crime” Hypothesis*, 55 STAN. L. REV. 1193, 1220-23 (2003). Ayres and Donohue suggest that if a right-to-carry law had an initial “announcement effect,” where potential criminals became scared of bystander or victim retaliation, but then the law led to increasing crime levels as more people possessed weapons, the hybrid model would capture the two effects. *Id.* at 1220. Similarly, if people initially increased seatbelt use because they expected high enforcement but then use regressed to earlier levels due to low enforcement, this model would capture that and indicate that the law had little or no expressive effect.

jurisdictions and runs from eight years before the law passed to six years after. These results are reproduced as Tables 2a, 2b, and 2c, respectively, and they are discussed in Part IV.

2. The Impact of Laws After Controlling for Enforcement

To test whether seatbelt laws have expressive power, I separate the simple deterrent effect of these laws from their expressive effect. I look to enforcement levels to see whether compliance is closely tied to punishment, as would be expected with a simple deterrence model, or whether compliance is also tied to the simple presence of the law, even controlling for enforcement levels, as would be expected from an expressive model. State enforcement levels are not collected in a single database. Therefore, I assembled a unique set of panel data by calling the Department of Public Safety, the Department of Motor Vehicles, or the Administrative Office of the Courts in each of the forty-nine states with seatbelt laws. For some states I obtained annual data on convictions for failure to wear a seatbelt; for some states I found out how many citations for failure to wear a seatbelt there had been in the entire state; and for some states I learned how many citations for failure to wear a seatbelt had been issued by the state highway patrol or state police. Some states had twenty years of citation or conviction data, and others only had a few years of data. (I dropped states with fewer than five years of data.) I obtained at least some usable data for twenty states, nine of which had state conviction data and nine of which had data on citations issued by the state police. (Two states had data on total tickets issued. I did not use these states in my regressions.) Because state officials noted in phone conversations that local police may be responsible for a large portion of citations issued, and because it is not clear what percentage of citations eventually turn into convictions, I run separate regressions for the “state conviction” states and for the “highway citation” states. I have included information on which states contributed what kinds of data in Appendix A.

Tables 3a through 3f present my regression results when I control for enforcement. The independent variables in these regressions always include a dummy indicating whether the state had a primary law, a dummy indicating whether the state had a secondary law, a dummy for the presence of the New York law (which takes a value of one for every year after 1984), and a continuous enforcement variable.⁹⁹ I calculate enforcement using a different measure in each table, so I can capture any way in which enforcement level might affect behavior. In Tables 3a and 3b, I express enforcement as the expected cost of driving without a seatbelt, by calculating the number of

99. As with the earlier regressions, some specifications include social controls, state and year fixed effects, and/or linear trends. These regressions no longer include a trend variable indicating change in slope; they are dummy models, not spline/dummy hybrids.

convictions (3a) or citations (3b), multiplied by the state's fine for failure to wear a seatbelt in that year, divided by the number of highway vehicle miles (in thousands) driven in the state during that year.¹⁰⁰ In Table 3c, I look exclusively at fine level as a measure of enforcement. In Tables 3d and 3e, I eliminate fine level from the calculus, and enforcement is calculated as the number of convictions (3d) or citations (3e), divided by the number of highway vehicle miles (in thousands) driven in the state during that year. In these tables, therefore, enforcement is just the likelihood of getting ticketed, not the expected cost of driving without a seatbelt. Table 3f investigates whether the results are robust to an alternate expression of the dependent variable by using the "Natural Log of Use" as the dependent variable.¹⁰¹

In each of the Table 3 regressions, I look to whether the coefficients on the "Primary Law" and "Secondary Law" dummy variables are robust to the addition of enforcement data. I discuss my results in Part IV.

3. The New York Law

As I have noted, the 1984 New York law garnered much press attention and controversy. Mentions appeared in the *New York Times*,¹⁰² *Washington Post*,¹⁰³ and even the *Guardian*.¹⁰⁴ If the passage of this law changed the behavior of people in states other than New York, it would speak powerfully to the ability of law to shape norms without threatening punishment. To investigate this, I look at two groups of states: states contiguous with New York, including Connecticut, New Jersey, Pennsylvania, and Vermont, and far away states with some cultural similarity to New York, including California, Oregon, and Washington. For each of these groups, I regress seatbelt use on whether the state had a primary law, whether the state had a secondary law, and whether the New York law had gone into effect. If the New York law had no expressive power, we might still see a significant effect on seatbelt rates in the

100. The number of citations or convictions comes from my communications with state agencies. The fines were determined by comparing current fine information from the Insurance Institute of Highway Safety ("IIHS"). See INS. INST. FOR HIGHWAY SAFETY, *supra* note 10 (providing historical IIHS Occupant Protection Fact Sheets and state bill histories). The number of highway vehicle miles in the state was calculated with data available from *Highway Statistic Series: Annual Vehicle Miles*, FED. HIGHWAY ADMIN., <http://www.fhwa.dot.gov/policyinformation/statistics/vm02.cfm> (last visited Apr. 21, 2011). I derived highway vehicle miles by summing the entries in the "rural interstate," "urban interstate," and "other freeways and expressways" columns. To avoid having one variable on both the right and left hand side of the regression, these tables do not account for rate of seatbelt use in their expression of the expected cost of being ticketed for failure to wear a seatbelt.

101. The coefficients in this regression represent not the percentage point change in use but rather the percentage change in use. For example, if the coefficient in front of an indicator variable is one, that does not mean that if the variable takes the value of one then use jumps up one hundred percentage points. Rather, it means that if the variable takes the value of one, use doubles.

102. See Oreskes, *supra* note 54.

103. *Seat-Belt Law in Effect*, WASH. POST, Dec. 1, 1984, at A6.

104. *Motoring: Belts Will Be Worn*, GUARDIAN (London), Dec. 17, 1984, at 22.

“near” states, as people drive across the border from New York and don’t immediately take off their seatbelts. However, we would be unlikely to see a substantial effect in West Coast states if New York’s law served no expressive function. My results appear in Appendix B as Table 4, and I discuss my coefficients, along with graphs illustrating seatbelt use in various locations before and after the New York law, in the next Part.

4. The Tennessee Law

When Tennessee first passed its seatbelt law in 1986, the law carried no fine, and Tennessee did not start imposing sanctions on unbelted occupants until 1994. The state of Washington also included no fine for seatbelt violations; however, Washington had a secondary law, and law enforcement officials deducted five dollars from the ticket for the other violation if the offender was wearing her seatbelt. This way, violations were not truly costless. The Tennessee law provides a rare opportunity to look at the effects of a law without sanction.

I regress seatbelt use in Tennessee on two indicator variables: one for the presence of the law with no fine, and one for the presence of the law with a fine. Table 5 in Appendix B presents my results, which are discussed in Part IV.

IV. Findings

My analysis consistently shows an expressive effect of seatbelt laws. Indicator variables for primary and secondary enforcement laws retain robust coefficients whether or not I control for enforcement, and I observed substantial effect of the New York law through both my regressions and line graphs. Analysis of the Tennessee law also suggests an expressive effect, although it is not conclusive. This Part discusses each of these findings in turn. It then discusses potential alternative explanations for my findings and explains why my conclusions are resilient to these challenges.

A. The Impact of Primary and Secondary Laws

My investigation of the overall impact of primary and secondary laws replicates the results of previous research demonstrating that states with primary laws have higher seatbelt use than states with secondary laws, and states with secondary laws have higher use than states with no law at all.

Table 1 in Appendix B shows the results of my regressions where the dependent variable is use, that is, the percentage of people who wear a seatbelt, and the independent variables include an indicator for the presence of a primary law and an indicator for the presence of a secondary law. The strongly significant coefficients on these regressions indicate that the passage of a

primary law, with all of the enforcement and publicity endogenous to it, raises seatbelt use by approximately eighteen to twenty-one percentage points, and a secondary law raises seatbelt use by approximately six to twelve percentage points. This is consistent with Cohen and Einav's 2003 study, which found a twenty-two percentage point increase in use from a primary law and an eleven percentage point increase in use from a secondary law.¹⁰⁵

My spline/dummy hybrid models confirm this result. Table 2a shows the coefficients for a hybrid model for eight states that changed from no law to a primary law: Connecticut, Hawaii, Iowa, New Mexico, New York, North Carolina, Oregon, and Texas. I have two slope variables: "Year, Relative to Law" is a trend variable that runs from -9 to 16, with 0 as the year the law was passed, and "Years After Law" is 0 every year up through the year of passage, and then increases by 1 every year. While the results for my slopes are ambiguous, the change in intercept at the time of the passage of the primary law remains robust to this change in model; depending on the specifications, the primary law caused a jump anywhere from eighteen to twenty-four percentage points. Figure 2a shows this jump.

Figure 2a: Use as a Function of Year, Relative to Law Passage (Data from All States with a Primary Law)

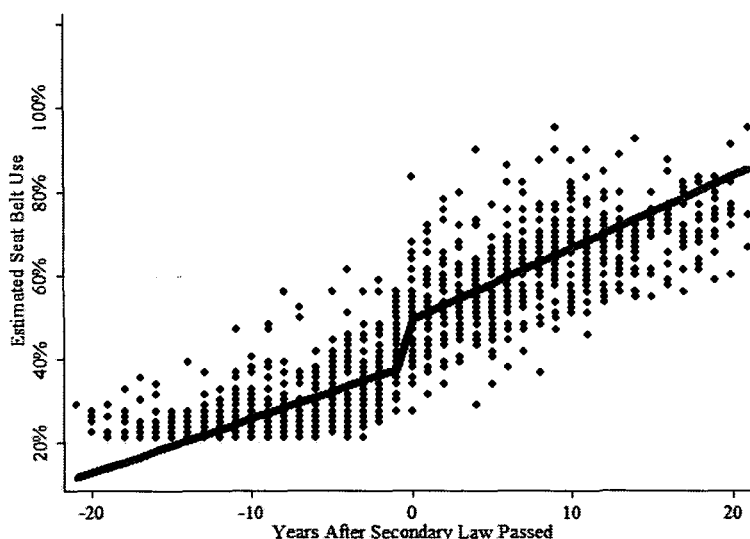


Note: Figure based on *Use(1)* in Table 2a.

105. Cohen & Einav, *supra* note 73, at 829.

Table 2b in Appendix B reveals the regression coefficients for a spline/dummy hybrid model for 41 states and the District of Columbia¹⁰⁶ that went from no law to a secondary law. As with the primary law, the secondary law coefficient is robust to this change in model from dummy to spline/hybrid, maintaining a coefficient of eight to twelve percentage points, depending on specifications. Figure 2b presents this model in graphical form.

Figure 2b: Use as a Function of Year, Relative to Law Passage (Data from All States with a Secondary Law)



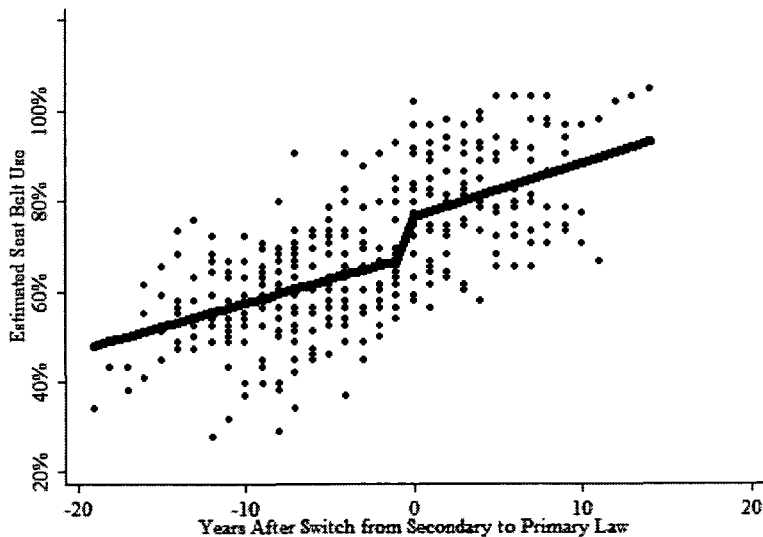
Finally, Table 2c looks specifically at the switch from a secondary law to a primary law, modeling this switch with a spline/dummy hybrid. I included seven jurisdictions in this regression: the District of Columbia, Indiana, Louisiana, Maryland, Michigan, New Jersey, and Oklahoma. Here, the coefficient on the variable primary law is not the total change caused by a primary law, but only the change relative to an earlier secondary law.

For specifications that include social controls, there is a significant change from a secondary law to a primary law of approximately the magnitude we would expect from comparing the individual effects of the laws. The first three models in my regression table, those without social controls, show a jump in

106. Alabama, Alaska, Arizona, Arkansas, California, Colorado, Delaware, the District of Columbia, Florida, Georgia, Hawaii, Idaho, Illinois, Indiana, Kansas, Kentucky, Louisiana, Maine, Maryland, Massachusetts, Michigan, Minnesota, Mississippi, Missouri, Montana, Nebraska, Nevada, North Dakota, Ohio, Oklahoma, Pennsylvania, Rhode Island, South Carolina, South Dakota, Tennessee, Utah, Vermont, Virginia, Washington, West Virginia, Wisconsin, and Wyoming.

use when the state switches from secondary enforcement to primary enforcement, but this jump is neither large nor significant. Other researchers, including Cohen and Einav, have found that switching from a secondary law to a primary law increases usage by about thirteen percentage points.¹⁰⁷ The insignificant coefficients in my regression are not particularly troubling. It is possible that in these seven states, social factors—those represented by my matrix of controls—were operating to increase seatbelt use before the law passage and then worked to decrease seatbelt use after the law passage. This could happen if, for example, more teenagers started driving in the state shortly after the law was passed. Allowing for social controls vastly improves the R-squared, the explanatory power of the model, indicating that the fourth and fifth specifications are likely more accurate representations of how the law affected use than the first and second. Figure 2c displays a fit that includes all states that switched enforcement mechanisms, not just the seven included in Table 2c.

Figure 2c: Use as a Function of Year, Relative to Law Upgrade (Data from All States That Switched from a Secondary Law to a Primary Law)



B. The Impact of Laws After Controlling for Enforcement

The findings up to this point largely replicate results from earlier studies, but even on their own, they speak to an expressive effect of law. In states with secondary laws, drivers cannot be ticketed solely for violating the seatbelt law.

107. Cohen & Einav, *supra* note 73, at 839.

Therefore, the expected sanction for failing to wear a seatbelt in the absence of other traffic offenses is zero. In addition, many police departments discourage “piling on,” or issuing tickets for more than one offense in a single traffic stop, making it less likely that a ticket will be issued for failure to wear a seatbelt at any given stop.¹⁰⁸ It also seems likely that drivers might be cognizant not only of their state’s enforcement mechanism, but also that secondary enforcement presents little threat of sanction. When New Jersey passed its secondary law, the *New York Times* railed against the legislature, calling the bill “toothless” and “worse than no bill at all.”¹⁰⁹ I am not the first person to imply that compliance with secondary enforcement seatbelt regulations implies some expressivism in law. In her article on expressive law, Funk cited the Cohen and Einav study as preliminary evidence of law’s expressive power.¹¹⁰

This argument would be strengthened considerably, however, either by showing zero enforcement in secondary law states or by showing that people’s reaction to the law cannot be explained by enforcement levels. The first is impossible to show. Some states with secondary laws have decidedly nonnegligible enforcement levels. For example, in 2002, Massachusetts issued over 100,000 tickets for failure to wear a seatbelt, or one ticket for every forty-five licensed drivers. By contrast, in the same year, North Carolina, a primary enforcement state, handed down one conviction for every thirty drivers. While this difference is not negligible, especially considering that North Carolina had close to 85% compliance whereas Massachusetts had less than 60% compliance,¹¹¹ it suggests that secondary laws may carry more than a symbolic threat of sanction. Instead, I control for enforcement levels to examine how much of the behavior is attributable to simple deterrence and how much is attributable to the expressive impact of the mere presence of the law. By showing that the law’s influence on compliance is significantly different from zero, even when I control for actual enforcement, I show that law has an expressive effect.

Tables 3a and 3b in Appendix B display regression coefficients for the expected cost of not wearing a seatbelt, the presence of a primary law, the presence of a secondary law, and the existence of the New York law. These regressions also include an enforcement variable: the amount of money paid for seatbelt tickets that year, divided by the number of highway miles driven in the state that year. In Table 3a, my measure of tickets is the number of convictions. As the regression tables show, the absolute effects of primary enforcement laws and secondary enforcement laws that we observed in the earlier regressions are

108. Telephone Interview with Michele Fields, *supra* note 77.

109. Editorial, *Auto ‘Safety’ Can Be Harmful*, N.Y. TIMES, Oct. 16, 1984, at A30.

110. Funk, *supra* note 6, at 137 (“In the case of seatbelt legislation, Cohen and Einav (2003) document that barely enforced seatbelt laws had an effect on seatbelt usage.”).

111. NAT’L HIGHWAY TRAFFIC SAFETY ADMIN., *supra* note 81.

highly robust to the addition of conviction data. While there is a significant correlation between enforcement level and seatbelt use, the primary law itself was still associated with an eighteen to twenty-three percentage point increase in seatbelt use, and secondary laws were associated with a nine to eighteen percentage point increase in use. (The apparent effect of secondary laws actually grew with the addition of this control data.) By contrast, enforcement that is one standard deviation above the mean 2007 level is associated with less than a seven percentage point increase in seatbelt use.

These numbers are somewhat less robust to highway citation data, my enforcement measure in Table 3b. While the primary law still has a strong, positive effect on seatbelt use, either the number of citations is negatively correlated with use or the secondary law has an insignificant effect on use, depending on the model specifications. In other words, at face value, these coefficients indicate that enforcement decreases compliance or that secondary laws have significant impact on their own. The large fluctuations in these coefficients, however, may be less of an indication that neither enforcement nor secondary laws matter than an indication that the fine level is not an important factor in affecting use. I include the price of a ticket in my enforcement variable here; if the fine level is not an important determinant of use, then this model may fail to explain variance in seatbelt use.

Indeed, in Table 3c, I find that fines are not a good predictor of seatbelt use. There does not appear to be a significant correlation between fine level itself and compliance. A more appropriate expression of enforcement, then, would be the expected chances of getting a ticket.

Tables 3d and 3e look at the expected chances of being convicted and cited, respectively. The coefficients for a primary law and secondary law remain robust to conviction data. While the impact of citations per vehicle mile is itself no more robust in Table 3e than it was in Table 3b, this new model does support an effect from the secondary law itself that is independent of enforcement level.

Another possible explanation for the less robust secondary law effect after I control for citation data (as opposed to a more robust effect with conviction data) is that this small group of states is not nationally representative. The low coefficient could be unrelated to the enforcement data and could instead just be a result of the specific states included in these regressions. This hypothesis is borne out, at least in part, when I run the regressions for the same states and years without enforcement data, as shown in Table 3g. While the coefficient for a secondary law is closer to the national average when I do not control for enforcement level, it is still significantly lower under two specifications than the eight to twelve percent we observe nationally. We may then be able to understand the inconsistent secondary law coefficients in Tables 3b and 3e as an artifact of the specific selection of states. It does not necessarily indicate that if enforcement is positively correlated with use the law does not affect use in

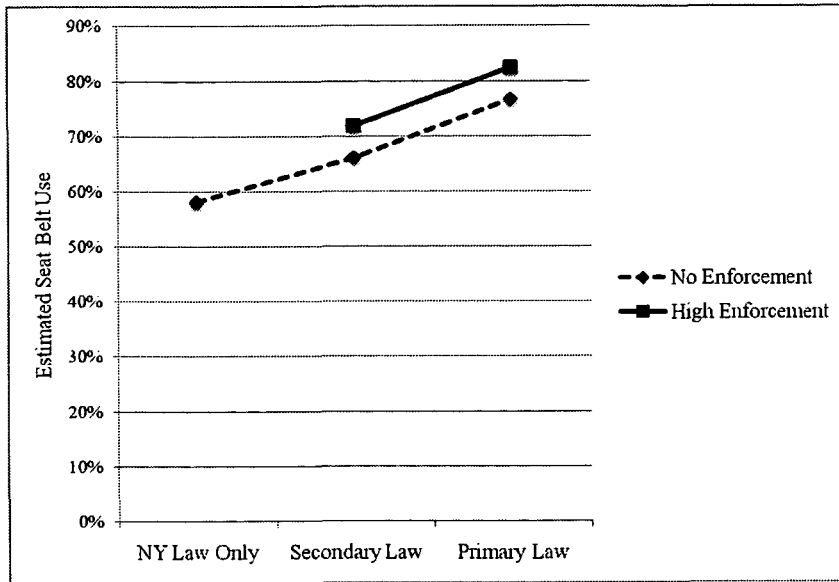
any other way. This interpretation is supported by the consistency of the primary and secondary law coefficients through all the regressions for states that provided conviction data.

The coefficients for primary and secondary laws in states with conviction data are also robust to an alternate expression of the dependent variable. When I substitute the natural log of use for use in my regressions, the coefficients for primary and secondary laws remain large and significant. In Table 3f, my dependent variable is the “Natural Log of Percent ‘Known’ Restrained in the FARS Data.” While expressing use as its natural log creates inexplicably negative coefficients for enforcement levels, the coefficients for primary laws and secondary laws, separate from enforcement levels, hold up as large and significant. This additional check further supports the conclusion that primary and secondary laws influence compliance through their expressive function.

Figure 3a, based on the first column of Table 3d, shows the strength of the expressive effect—the effect of the presence of the law—relative to the effect of enforcement. One line shows estimated seatbelt use without a law, with a secondary law, and with a primary law, holding enforcement at zero. The other shows estimated seatbelt use with high enforcement, one standard deviation above the 2007 average.¹¹² The “high enforcement” line sits above the “no enforcement” line, showing that enforcement does have an effect on seatbelt use. However, the differences within a line are even greater, demonstrating that the expressive effect is larger than the effect of enforcement.

112. Because there can be no enforcement without a law, I do not include a “NY Law Only” point on the “High Enforcement” line.

Figure 3a. Use in States with No Law, a Secondary Law, and a Primary Law (First Without Enforcement and Then with Enforcement One Standard Deviation Above Average)



Note: Error bars represent 95% confidence intervals. Estimations are calculated using the first specification in Table 3d in the year 2007.

All of the regressions in this Part together show that both primary laws and secondary laws have a robust effect on seatbelt use, even when enforcement level is included in the regression. This implies that something beyond the tangible cost of getting caught is driving behavior, something that consistently coincides with the law and is more powerful for a primary law than a secondary law. It seems likely that the law itself communicates the standards of the state and the community, and that people take primary laws more seriously than secondary laws not because they think they will be caught, but rather because a primary law expresses a seriousness that a secondary law does not.

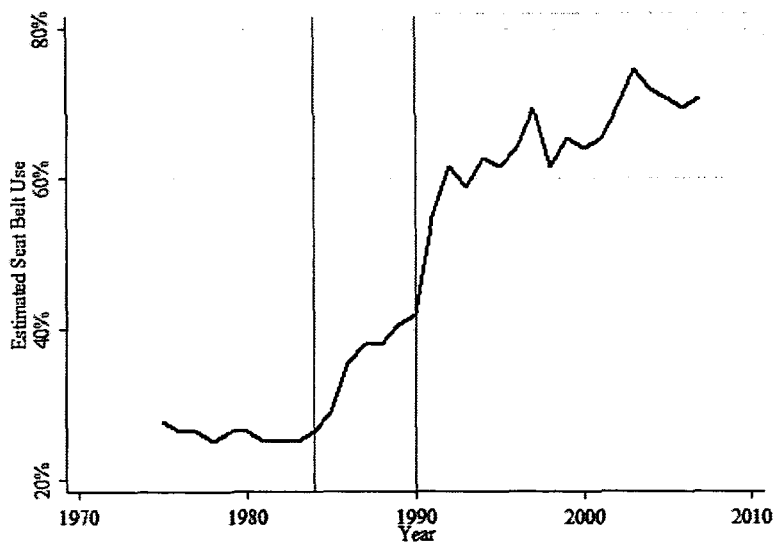
To bolster this theory, however, it is useful to look at more explicit, if less statistically robust, expressive effects of seatbelt laws.

C. The New York Law

I find that the New York law had a substantial impact on seatbelt use around the country, even in states that did not adopt a law shortly after New York. The most dramatic change in seatbelt use after the New York law occurred, of course, in New York itself. Figure 4a sufficiently illustrates the change in seatbelt use after the passage of the law.

Figure 4a: Historical New York Use: Law Passed December 1984

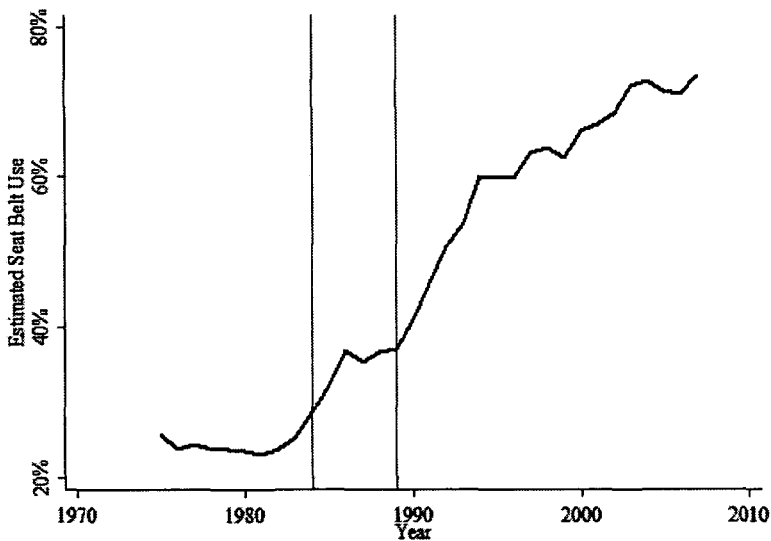
A similar visualization can show the impact of New York's law on use in other states. Figure 4b shows seatbelt use in Arizona, with reference lines indicating the year before the New York law passed and the year before Arizona's own secondary law passed.

Figure 4b: Historical Arizona Use: Law Passed January 1991

In Table 4 in Appendix B, I present the results of my regression comparing the change in use after the New York law in states near New York with the change in states that might align culturally with New York but are located on the opposite coast. I find the New York law had at least as large an impact on the Western states as it did on those next to New York. California and Washington, however, adopted laws within two years of New York, so it is possible that the change in seatbelt use in late-adopter Oregon was partially due to traffic from California or Washington. Still, the change attributable to out-of-state laws for both the Eastern and Western states is large and significant.

Figure 4c shows the impact of the New York law on use for sixteen “late adopting” states who passed their seatbelt law in 1990 or later.

Figure 4c: Use in States That Adopted Laws After June 1990, Before and After New York Law



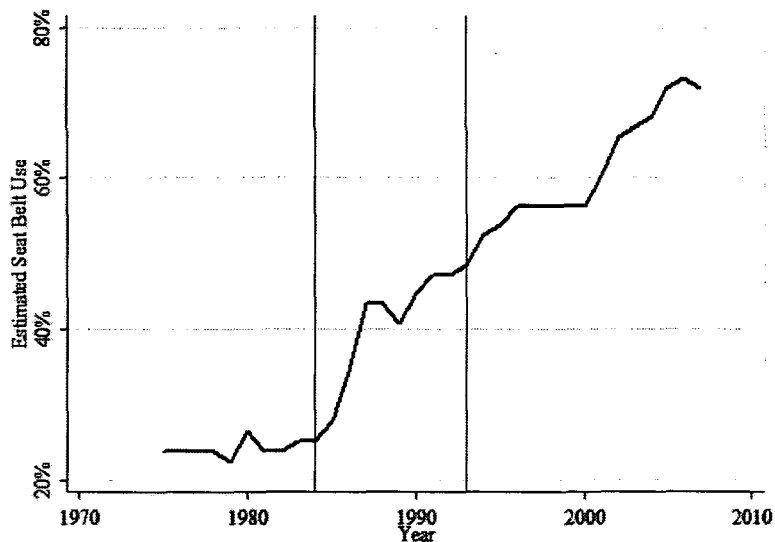
Note: Driver-weighted average of AL, AK, AZ, AR, DE, KY, ME, MA, MS, NE, ND, OR, RI, SD, VT, and WV. Reference lines appear at 1984 and 1989.

This graph shows that seatbelt use rose substantially in these states, even before they passed their own laws. A regression on only the late-adopting states for the years before 1990 confirms this observation. Controlling for my matrix of social controls and state fixed effects, I find that the New York law raised seatbelt use in these states by 7.4% ($\sigma = 0.011$). While the precise national effect of the New York law might be difficult to estimate, all of these results taken together show that the law altered behavior around the country.

D. The Tennessee Law

Table 5 in Appendix B shows how much seatbelt use changed in Tennessee when its law without a fine was passed and when the state legislature instituted a fine. While the addition of a fine had a strong impact on seatbelt use in Tennessee, the law without a fine also takes a significant coefficient in the regression. Unfortunately, it is difficult, if not impossible, to use this method to differentiate the impact of this 1986 law from the national impact of the New York law and the national discourse about seatbelt laws. Figure 5 shows Tennessee seatbelt use as a function of time. Reference lines mark the year before the first Tennessee law was passed (1985) and the year before the state started imposing fines (1993).

Figure 5: Tennessee Seatbelt Use Before and After the Law and Fine



Note: Reference lines appear at 1985 and 1993, just before the passage of the Tennessee law and fine, respectively.

E. Possible Alternative Explanations

My results consistently point to an expressive function of seatbelt laws; however, several alternative explanations could explain my data. In this Section, I address these alternatives and argue that my conclusions remain robust to the challenges they pose.

1. The Impact of Primary and Secondary Laws

Primary laws have a greater impact on use than secondary laws, and secondary laws have a greater impact than no laws at all. While the robust and substantial secondary law effect points to legal expression—drivers who comply with every law except the seatbelt law cannot be pulled over; therefore, if they buckle up, it should be for another reason—in reality, police ticket drivers in secondary enforcement states. If states with primary laws, on average, ticket more than states with secondary laws, which in turn ticket more than states with no laws, the imperative theory of law could explain my results. When I control for enforcement, however, I eliminate this possibility.

2. The Impact of Laws After Controlling for Enforcement

a. Inaccurate Perceptions of Enforcement

By controlling for enforcement, I attempt to refute the argument that the imperative theory of law fully explains compliance with seatbelt laws. However, if people have perceptions about their likelihood of getting caught that do not precisely track reality, these misperceptions could explain my findings without implicating an expressive function of law. There is evidence that perceived likelihood of being ticketed for noncompliance is higher in high-use states than in low-use states,¹¹³ possibly due to high visibility blitz enforcement campaigns. This is the strongest counterargument to my conclusions. For two reasons, however, it seems unlikely that misperceptions about enforcement account for the entirety of the difference between compliance and actual enforcement.

First, preliminary analysis of survey data—the same survey that indicated people in high use states perceive high enforcement—suggests that primary laws and secondary laws retain their effect after controlling for perceived likelihood of being ticketed. I obtained the data file for NHTSA's 2007 Motor Vehicle Occupant Safety Survey ("MVOSS"),¹¹⁴ the nationally representative survey that included a question on perceived enforcement.¹¹⁵ I ran two sets of regressions with use as the dependent variable and MVOSS perceived enforcement as a right-hand variable. In one set, use is measured by the participant's response to MVOSS questions asking how frequently she wears a

113. HEDLUND ET AL., *supra* note 47, at 21 tbl.13.

114. 2007 Motor Vehicle Occupant Safety Survey, NAT'L HIGHWAY TRAFFIC SAFETY ADMIN., <http://www.nhtsa.gov/Driving+Safety/Research+&+Evaluation/2007+Motor+Vehicle+Occupant+Safety+Survey> (last visited Apr. 10, 2011).

115. "Q.53 Assume that you do not wear your seatbelt AT ALL while driving over the next six months. How likely do you think you will be to receive a ticket for not wearing a seatbelt?" Answer options included "very likely," "somewhat likely," "somewhat unlikely," and "very unlikely." *Id.* I recoded these answers as 4, 3, 2, and 1, respectively.

seatbelt, and observations are individual survey participants (Table 6a). In the other, use is derived from FARS data (as in the rest of this Note), observations are states, and perceived enforcement is the average response in that state to the question about perceived enforcement (Table 6b).

In these regressions, primary laws and secondary laws have an effect on use, controlling for perceived enforcement. I have only one year of data for these regressions, as data files were not available for earlier MVOSS surveys. Therefore, my coefficients for primary law and secondary law are not nearly as significant as they are in my regressions on the multi-year panel data. Still, they maintain positive values of a comparable magnitude. There are several other reasons to be concerned about these regressions. The 2007 MVOSS was nationally representative, surveying more people from larger states. I have enforcement data from relatively few states, so the two last regressions in Table 6a seriously overweight large states, and the four final regressions in Table 6b contain only ten observations each. Furthermore, because the survey was designed to be nationally representative, not representative within each state,¹¹⁶ imperfect sampling may taint my Table 6b results. Also, regressions containing both actual enforcement and perceived enforcement on the right-hand side over-control for the “deterrence” variable and therefore may not be particularly informative.

The strongest evidence for robustness to perceived enforcement here is the second column in Table 6a. This regression, run on individual survey responses, has “Stated Personal Frequency of Seatbelt Use” as its dependent variable, and both “Law Type” and “Individual Perceived Enforcement” as independent variables. Even controlling for perceived enforcement, I find significant coefficients for law type. This shows an expressive effect of law that cannot be explained either by actual expected punishment, as shown earlier, or by perceived expected punishment.

Second, and perhaps more convincingly, my results are robust to the addition of a dummy variable for whether the state had a high-visibility Click it or Ticket (“CIOT”) campaign—an enforcement blitz designed to increase seatbelt use—in a certain year.¹¹⁷ Table 6c replicates Table 3d, which controlled for enforcement, but it also includes an indicator variable for CIOT campaigns. My regression results do not change significantly.

While perceived enforcement no doubt affects seatbelt use—the CIOT variable is strongly correlated with the actual enforcement variable, which likely explains CIOT’s insignificant coefficient—these pieces of evidence

116. E-mail from Scott Roberts, Research Psychologist, Office of Behavioral Safety Research, Nat’l Highway Traffic Safety Admin., to author (Apr. 30, 2010, 09:42 EST) (on file with author).

117. See NAT’L HIGHWAY TRAFFIC SAFETY ADMIN., CAMPAIGN HISTORY FACT SHEET, available at <http://www.nhtsa.gov/staticfiles/communications/pdf/ciot-history.pdf> (listing the states that used CIOT initiatives in each year prior to 2003).

together indicate that it is not doing all of the work. Primary and secondary laws have a large and highly significant impact on seatbelt use, independent of enforcement and, it appears, independent of perceived enforcement.

b. Endogeneity in the Model

The dependent variable, use, could influence the independent variable, enforcement, instead of the other way around. If people comply with the law in anticipation of enforcement, police officers will not need to issue citations. The resulting high compliance and low enforcement would yield regressions that attribute compliance to the mere presence of the law, instead of attributing low enforcement to high compliance.

I can partially guard against this problem by lagging the enforcement variable by one year. In a lagged-enforcement regression, I correlate seatbelt use in year t (say, 2004) with enforcement in year $t-1$ (say, 2003). Because compliance in 2004 cannot affect enforcement in 2003, I have removed the endogeneity from my regression. Table 6d displays the coefficients for these regressions and shows that my results do not change when I lag the enforcement variable. Primary and secondary laws still have an independent effect.

This is not a perfect fix. States with extremely high compliance will necessarily have low enforcement, since I measure enforcement as convictions per miles driven, not convictions per noncomplying vehicle occupants. If a state has 99.9% use, then even if every noncomplying driver is ticketed, the police will still issue relatively few tickets, and high compliance will be attributed to the presence of the law in my regressions. Unfortunately, I cannot replace enforcement with “convictions per noncomplying drivers,” because I may not use the same data—percent compliance—on both sides of a regression. Absent a strong instrumental variable,¹¹⁸ lagging enforcement is the best option available.

c. An Imperfect Enforcement Measure

Instead of indicating an expressive effect, the positive coefficients on my primary law and secondary law indicator variables could be compensating for an imperfect enforcement measure. No single enforcement measure can capture the entire deterrent component of seatbelt laws. People may be deterred by a

118. An instrumental variable only influences the dependent variable in a regression by influencing an independent variable. The instrumental variable substitutes for the independent variable in the regression, and if the coefficient on the instrumental variable is significant, we can infer that the coefficient on the independent variable would be significant. I obtained data on the number of highway patrol officers in each state. However, those data did not have a positive correlation with actual enforcement, and could not be used as an instrument.

“true enforcement” that is, for example, part expected cost of failing to wear a seatbelt and part ticketing frequency. To the extent that my enforcement variable is not perfectly correlated with “true enforcement,” my indicator variables for primary laws and secondary laws may provide additional information about enforcement. At the extreme, if my enforcement variable had no correlation with “true enforcement,” we would expect a large, positive coefficient on the law indicator variables, even if there were no expressive effect.¹¹⁹

It is unlikely that this residual effect accounts for my results, however. My coefficients are robust to several expressions of enforcement—both tickets and citations, and both expected cost and expected number of tickets. Also, my results indicate that the law itself has a larger effect than enforcement. I would need a very weak correlation between measured enforcement and “true enforcement” to find such a large effect. Because my enforcement variable is based off of actual citation and conviction data, it is unlikely that enforcement tracks “true enforcement” that poorly.

d. Common Causation of Law Passage and Increased Use

It is possible that a hidden variable—maybe a galvanizing event, such as a media campaign or a celebrity car crash—independently caused both the law passage and the increase in seatbelt use in each state. While this is a common concern in panel data models,¹²⁰ a hidden variable is unlikely to be driving my results. First, by five years after the New York law passed, thirty-four states had enacted seatbelt laws.¹²¹ This indicates that there was a bandwagon effect: instead of passing these laws for independent, hidden reasons, states passed laws because other states were passing them. Also, the best-known motivating factor for passing primary seatbelt laws is the promise of federal highway funding,¹²² which is external to the state and would likely have no impact on individual behavior. Finally, where I include state and year fixed effects, these account for state-specific and year-specific differences.¹²³

3. The New York Law

I find large coefficients for the effect of the New York law, which in some regressions imply that the New York law alone caused an increase of over

119. I thank Yair Listokin for raising this point.

120. E-mail from John J. Donohue III, C. Wendell and Edith M. Carlsmith Professor of Law, Stanford Law Sch., to author (Jan. 19, 2011, 02:22 EST) (on file with author).

121. See INS. INST. FOR HIGHWAY SAFETY, *supra* note 10.

122. See *supra* notes 60–61 and accompanying text.

123. I thank Dan Kahan for pushing me to think about the endogeneity and hidden variable concerns and Chris Griffin for helping me to address them.

thirty percentage points in seatbelt use. The graphs are also dramatic, showing a sharp spike after the law was passed. While I cannot precisely determine the impact of the New York law in percentage points, I can address other possible explanations for the effect to show that the law was, indeed, significant.

First, it might appear from my graphs that the change in use started before the legislature passed the law, so while we may not know exactly what caused the change, it could not have been the New York law. The sharp uptick before the reference line, however, simply connects the low use of 1983¹²⁴ with the higher use of 1984. The New York law did not go into effect until late December 1984; however, the legislature agreed to pass the law in June of that year, and news of the law appeared on the front page of the *New York Times* at that time.¹²⁵ It is not surprising that we would see an impact of the impending law in 1984, and since there were no fines imposed in 1984, the change we see in that year is an expressive effect.

Second, it is possible that the impact I attribute specifically to the New York law actually came from every state that passed a law within a few years of 1984 or from the national discussion and controversy about seatbelt laws. It also may have been a response to NHTSA's threat to mandate automatic restraints in every car if more states did not pass seatbelt laws.¹²⁶ These possibilities do nothing to undermine my point and may strengthen it. If a large pattern of lawmaking changes behavior, this change is powerful evidence that the legislators successfully expressed social values and changed social norms through their legislation. In other words, almost any explanation for the change in national seatbelt use following passage of the New York law must contain an element of the expressive power of law.

I can also eliminate several other conceivable explanations for the rise in seatbelt use in 1984. Great Britain began requiring seatbelt use in 1983,¹²⁷ but it is unlikely that this law had an effect (an effect which would be entirely expressive, if it exists) because Canada passed its law in the 1970s, and American seatbelt use did not budge.¹²⁸ NHTSA started mandating seatbelts be installed in all cars in the 1960s,¹²⁹ so there was no new availability of seatbelts at the time the New York law was passed. It is also unlikely that the effect can be attributed to campaigns in favor of seatbelt use unrelated to the law. NHTSA had emphasized seatbelt use for years prior to the New York law's passage, but

124. Even in Figure 4c, 1983 use is below 1975 use.

125. See Oreskes, *supra* note 54.

126. Donohue, *supra* note 33, at 461-62.

127. J.G. Avery, *Seatbelt Success: Where Next?*, 288 BRIT. MED. J. 662, 662 (1984).

128. Allan F. Williams & Adrian K. Lund, *Seatbelt Use Laws and Occupant Crash Protection in the United States*, 76 AM. J. PUB. HEALTH 1438, 1438 (1986).

129. Donohue, *supra* note 33, at 460.

its efforts had a small effect, if any, on seatbelt usage.¹³⁰ Finally, no major celebrities were killed in car crashes in either 1984 or 1985.¹³¹ The effect of the New York law appears to be genuine.

4. The Tennessee Law

My investigation of the Tennessee law admittedly provides only weak evidence for an expressive effect. It is not immediately clear that Tennessee's graph looks different from those states that did not have a law before 1990.¹³² More delicate statistical analysis might be able to tease out an effect in the Tennessee law by looking at the moment the state imposed a fine and comparing the change in Tennessee seatbelt use to the change in other states that enacted a law around the same time. For now, we can say that there appears to be some expressive effect to Tennessee's law without a sanction, but this may actually be the product of a more general expressive effect of all of the seatbelt laws, especially the highly publicized New York law, passed in the mid-1980s.

V. Implications

This Note has provided four pieces of evidence for the expressive power of seatbelt laws. First, this Note has shown that secondary laws, despite having zero penalty for otherwise obedient drivers, have a large and significant impact on behavior. Second, it has shown that both primary and secondary laws have a large and significant impact on seatbelt use, even controlling for actual enforcement level. These two findings indicate that seatbelt laws alter behavior in ways that do not fit with a simple imperative theory: compliance exceeds expected sanction. As scholars include all mechanisms through which law affects behavior other than simple deterrence under the term "expressive law,"¹³³ these results show an expressive function of law. Third, this Note has revealed the enormous impact that the New York law and the surrounding discussion had on seatbelt use in other states. As this law did not threaten drivers outside of New York State with punishment but did communicate social values and safety risks, any out-of-state effect of the New York law would be expressive. Finally, this Note has provided evidence that an early Tennessee law that did not provide for a sanction raised seatbelt use in Tennessee. A law

130. *Traffic Fatalities Increase in 1984*, SPARTANBURG HERALD-J. (Spartanburg, S.C.), Dec. 28, 1984, at C2 available at <http://bit.ly/d6YSqd>.

131. *Celebrity Automobile Accidents: Famous People Who Have Died in Car Crashes*, CELEBRATE TODAY, <http://www.celebratetoday.com/autoc celeb.html> (last visited Apr. 21, 2011).

132. Compare *supra* Figure 4c, with *supra* Figure 5.

133. See, e.g., Dharmapala & McAdams, *supra* note 14; Funk, *supra* note 6.

without punishment is classically expressive—it contains no deterrence mechanism—so any effect indicates expressive power.

This study provides much-needed empirical support for the existence of an expressive function of law by finding such an effect in seatbelt laws. While its findings are broad—they do not explicitly test whether this effect is attributable to, say, expression of expected social costs or safety risks—this study does support specific accounts of the expressive function of law and call others into question. Sunstein and Lessig’s “meaning” account, McAdams’s “attitudinal” account, Cooter’s “Pareto self-improvement” account, and McAdams and Dharmapala’s “superior legislative information” account are all consistent with this study’s results, and this study therefore lends a measure of support to these theories.

Sunstein suggests that the meaning account of expressive law—that a law can change the social meaning of an action—applies to laws governing risk-taking behavior. He notes that people, especially young people, often engage in risk-taking behavior for its reputational effects.¹³⁴ If a law successfully expresses condemnation for those who engage in the risk-taking behavior, however, the meaning of the action can change from “independence” to “weakness” or “stupidity.”¹³⁵ A seatbelt law could change the social meaning of driving without a seatbelt, either in state or out of state, so my findings are consistent with the meaning account.

Similarly, McAdams’s “attitudinal account,” which argues that certain laws convey society’s attitude toward an action, predicts an expressive effect of seatbelt laws. Because seatbelt laws have been well publicized, legislatures can be held accountable for them;¹³⁶ they are therefore more likely to represent true public opinion than laws passed without publicity. While my findings regarding the effects of the New York law do not provide strong support for this theory (McAdams suggests that local laws best express local attitudes), all of my findings on the in-state impact of primary laws and secondary laws support the attitudinal account.

An expressive effect of seatbelt laws also jibes with Cooter’s idea of “Pareto self-improvement,” which argues that people will change their preferences when the resulting observable change in character creates opportunities that are superior by both new and old preferences. A seatbelt law can create or communicate expected social costs of not wearing a seatbelt. It can also encourage a person to internalize a social norm of seatbelt wearing. With either of these outcomes, it will be personally costly to violate the norm, and it will behoove the person to change her preferences. While this theory,

134. See Sunstein, *supra* note 5, at 2034-35.

135. See *id.* at 2035.

136. See *supra* Part III.

again, lines up best with my findings of in-state expressive effects, all of my results are consistent with it.

The New York finding lends support to McAdams and Dharmapala's idea that laws can express a legislature's superior information about risk. If the New York law and surrounding discussion expressed expert consensus that seatbelts are an important safety measure deserving government mandates, we would expect to see the out-of-state effects that I observe.

Because these theories are not mutually exclusive—scholars acknowledge that a law can have multiple expressive mechanisms—my study does not support one of these theories over others. Nor does it eliminate theories such as Cooter's focal point account, which suggests that a law can solve collective action problems by indicating how other people will behave. Seatbelt laws are not public goods laws, and therefore, Cooter's account is inapplicable.

My findings concerning the New York law specifically indicate, however, that seatbelt laws are classically expressive of either attitudes or risks, and any expressive theory that relies on people being personally subject to a law's sanction does not sufficiently explain the expressive function of seatbelt laws. While there may be long-term deterrence at work where initial deterrence leads to subconscious habit, the national effect of the New York law indicates that this cannot be the only mechanism at work. Similarly, while some people may avoid mortality anxiety by buckling up simply because it is the law, people who used seatbelts after New York passed its law but before their own states did buckled for another reason. My findings therefore show that laws actually can express a message.

My findings also counsel against certain accounts. Theories suggesting that laws backed by a fine cannot have any expressive effect, because people will understand a fine to be the price of an otherwise acceptable action, are incorrect, or at least they are not broadly applicable. Because I find that high-enforcement primary laws have a larger expressive effect than low-enforcement secondary laws, my study instead supports the idea that punishments (including fines) enhance the expressive effect by indicating that an act is morally wrong.

While my study contributes to the descriptive discussion of expressive law, it perhaps holds the greatest value for scholars such as Sunstein and Kahan who espouse normative theories of expressive law, advocating for legislators and administrators to use law's expressive function to formulate effective policy. It is only worthwhile to argue about how we should capitalize on law's expressive effects if these effects are observable. My research is a case study, and I cannot confidently extrapolate the expressive function of seatbelt laws to all other laws. By supplying empirical evidence that certain laws have an expressive function, however, this Note supports the normative mission of these scholars.

Conclusion

This Note has shown that seatbelt laws have an expressive effect and that this effect is greater for primary laws than secondary laws. This Note has thereby demonstrated that law can have an expressive function and that this function is enhanced, not diminished, by a heightened threat of sanction. Further research could both examine the mechanisms of seatbelt laws' expressive function in more detail and use other case studies to develop a more comprehensive understanding of how expressive law works. Ideally, future study in this area would more closely investigate the Tennessee law to tease out its effect from the national increase in seatbelt usage that came with the New York law. Also, future studies would add value by closely examining the techniques and effectiveness of different enforcement, publicity, and visibility strategies in order to better separate out citizens' knowledge of the law from their expectation that they will be punished for noncompliance. Future investigations of expressive law should attempt, as Funk did, to find other laws with zero expected sanctions, and to determine whether these laws actually shape behavior. Finally, future empirical work should tease out the nuances of the expressive function of law to determine what features of a law maximize its expressive effect. This additional research would add to this Note's contribution: an empirical foundation that supports and directs scholars and policymakers looking to shape human behavior through law's expressive effect.

Appendix A: Sources of State Data

Alabama: State trooper citation data from the Department of Public Safety.

Idaho: Conviction data from Office of Highway Operations and Safety.

Illinois: Citation data from the State Police.

Indiana: Citation data from the State Police.

Kentucky: Citation data from the State Police.

Maine: Conviction data from the Bureau of Motor Vehicles.

Massachusetts: Citation data from the Merit Rating Board—the total number of citations issued by the commonwealth and received by the Merit Rating Board, not just from the State Highway Patrol.

Minnesota: Conviction data from the Supreme Court.

New Mexico: Conviction data from the Judicial Information Division. This data does not include municipal courts.

New York: Conviction data from the Department of Motor Vehicles.

North Carolina: Conviction data from the Administrative Office of the Courts.

Ohio: Highway Patrol citation data from the Ohio State Highway Patrol public affairs unit. These data cover only five years.

South Carolina: Citation data from the State Highway Patrol.

South Dakota: Conviction data from the Unified Judicial System. Data is for South Dakota Fiscal Years, which begin in July. Numbers for a calendar year are half of the earlier fiscal year and half of the later fiscal year.

Tennessee: Conviction data from the Department of Safety.

Texas: Citation data from the Department of Public Safety data for State Police.

Vermont: Citation data from the Court Administrator's Office.

Virginia: Conviction data from the Department of Motor Vehicles.

Wisconsin: Conviction data for the entire state from Department of Motor Vehicles records via the Wisconsin Safety Patrol.

Wyoming: State Highway Police citation data for both seatbelt and child restraint violations.

Appendix B: Regression Tables

Each column contains a different model specification, described by the “yes” and “no” cells in that column. For example, in Table 2a, the third specification contains year fixed effects—consistent differences between years—while the second specification does not.

“Primary Law,” “Secondary Law,” and “NY Law” are indicator variables. The coefficient on each of these variables indicates the percentage point increase in seatbelt use corresponding to the presence of that particular law. A coefficient of .18 indicates an 18 percentage point increase.

“Enforcement” is a variable showing how strictly a state ticketed. It is calculated differently in each table where it appears.

“Social Controls” indicates whether that specification controlled for per capita ethanol consumption, personal income per capita, percent of the population between ten and nineteen years old, state population density, and the percent of drivers from out of state.

“Linear Trends” accounts for the linear increase in use in each state—when I include linear trends, if certain states have especially rapid or slow growth in use for reasons unrelated to the law, growth will not be mistakenly attributed to the law.

“State Fixed Effects” and “Year Fixed Effects” account for consistent differences between states across all years and consistent differences between years across all states, respectively.

T-values are in parentheses. A t-value of 1.96 or higher indicates that the regression coefficient is significant at the 5% level: there is no more than a 5% chance of observing a coefficient at least this extreme if the null hypothesis is true and the “true” coefficient is zero. Coefficients significant at the 5% level are in bold. Standard errors are clustered at both the state and year level.

The R-squared value notes how much of the variance in the outcome variable is explained by the model. All R-squared values are adjusted if Stata produced an adjusted R-squared.

Table 1: The Effect of the Presence of a Primary Law, a Secondary Law, and an Out-of-State Law on Seatbelt Use

	<i>Use (1)</i>	<i>Use (2)</i>	<i>Use (3)</i>	<i>Use (4)</i>	<i>Use (5)</i>
Primary Law	.185 (8.94)	.384 (11.03)	.203 (7.31)	.182 (8.97)	.178 (8.24)
Secondary Law	.066 (4.95)	.223 (8.59)	.118 (6.49)	.069 (5.28)	.106 (6.33)
NY Law	.389 (24.13)	.132 (5.53)	.020 (1.04)	.223 (2.68)	.050 (2.71)
Social Controls	No	No	No	Yes	Yes
Linear Trends	No	No	Yes	No	Yes
State & Year Fixed Effects	Yes	No	Yes	Yes	Yes
R-squared	0.933	0.782	0.935	0.936	0.941

Note: The dependent variable is estimated seatbelt use, derived by regressing survey data provided by Liran Einav and Alma Cohen on FARS data and using FARS data to predict actual use. Regressions are for all fifty states and the District of Columbia from 1975 to 2007. Standard errors are clustered at the state level. T-values are in parentheses. Coefficients significant at the 5% level are in bold.

Table 2a: Spline/Dummy Hybrid Models for Primary Laws

	<i>Use (1)</i>	<i>Use (2)</i>	<i>Use (3)</i>	<i>Use (4)</i>	<i>Use (5)</i>
Primary Law	.235 (5.26)	.239 (8.53)	.186 (4.68)	.185 (4.44)	.218 (4.88)
Year, Relative to Law (-9 to 16)	.011 (2.71)	.009 (3.40)	-.003 (-0.93)	.011 (2.64)	.011 (2.67)
Years After Law (0 to 16)	.008 (2.16)	.011 (4.06)	-.024 (-3.63)	-.034 (-5.88)	-.029 (-4.15)
Weighted	No	Yes	No	No	Yes
Social Controls	No	No	No	Yes	Yes
Year Fixed Effects	No	No	Yes	Yes	Yes
R-squared	0.827	0.897	0.906	0.929	0.955

Note: The dependent variable is estimated seatbelt use, derived by regressing survey data provided by Liran Einav and Alma Cohen on FARS data and using FARS data to predict actual use. Regressions are for eight states (CT, HI, IA, NM, NY, NC, OR, TX) from nine years before their laws were passed through sixteen years after their laws were passed. Weighting is by number of licensed drivers, as reported by the Federal Highway Administration. Standard errors are clustered at the state level. T-values are in parentheses. Coefficients significant at the 5% level are in bold.

Table 2b: Spline/Dummy Hybrid Models for Secondary Laws

	<i>Use (1)</i>	<i>Use (2)</i>	<i>Use (3)</i>	<i>Use (4)</i>	<i>Use (5)</i>
Secondary Law	.108 (3.96)	.114 (5.79)	.081 (5.71)	.082 (6.64)	.081 (4.00)
Year, Relative to Law (-11 to 6)	.013 (4.98)	.010 (5.12)	.004 (1.50)	.003 (1.71)	.004 (1.13)
Years after Law (0 to 6)	.004 (0.76)	.011 (5.69)	-.001 (-0.21)	-.001 (-0.26)	.002 (0.34)
Weighted	No	Yes	No	No	Yes
Social Controls	No	No	No	Yes	Yes
Year Fixed Effects	No	No	Yes	Yes	Yes
R-squared	0.688	0.756	0.787	0.829	0.869

Note: The dependent variable is estimated seatbelt use, derived by regressing survey data provided by Liran Einav and Alma Cohen on FARS data and using FARS data to predict actual use. Regressions are for forty states and the District of Columbia (AL, AK, AZ, AR, CA, CO, DE, DC, FL, GA, ID, IL, IN, KS, KY, LA, ME, MD, MA, MI, MN, MS, MO, MN, NE, NV, ND, OH, OK, PA, RI, SC, SD, TN, UT, VT, VA, WA, WV, WI, WY) from eleven years before law passage to six years after. Weighting is by number of licensed drivers, as reported by the Federal Highway Administration. Standard errors are clustered at the state level. T-values are in parentheses. Coefficients significant at the 5% level are in bold.

Table 2c: Spline/Dummy Hybrid Models for Switch from Secondary to Primary

	<i>Use (1)</i>	<i>Use (2)</i>	<i>Use (3)</i>	<i>Use (4)</i>	<i>Use (5)</i>
Primary Law	.032 (2.44)	.049 (35.11)	.075 (2.50)	.095 (2.89)	.111 (3.97)
Year, Relative to Switch (-8 to 6)	.019 (3.52)	.017 (3.50)	-.044 (-3.72)	-.015 (-0.38)	.008 (0.19)
Years After Switch (0 to 6)	.001 (0.10)	-.002 (-0.38)	-.003 (-0.17)	.001 (0.06)	-.016 (-1.05)
Weighted	No	Yes	No	No	Yes
Social Controls	No	No	No	Yes	Yes
Year Fixed Effects	No	No	Yes	Yes	Yes
R-squared	0.395	0.414	0.660	0.763	0.782

Note: The dependent variable is estimated seatbelt use, derived by regressing survey data provided by Liran Einav and Alma Cohen on FARS data and using FARS data to predict actual use. Regressions are for seven jurisdictions (DC, IN, LA, MD, MI, NJ, OK) from eight years before the switch until six years after. Weighting is by number of licensed drivers, as reported by the FHA. Standard errors are clustered at the state level. T-values are in parentheses. Coefficients significant at the 5% level are in bold.

Table 3a: Effect of Cost of Convictions per State

	<i>Use (1)</i>	<i>Use (2)</i>	<i>Use (3)</i>	<i>Use (4)</i>
Enforcement	.000262 (2.81)	.000281 (2.74)	.000175 (1.11)	.000104 (0.46)
Primary Law	.183 (5.14)	.230 (4.45)	.203 (5.86)	.213 (9.83)
Secondary Law	.092 (3.11)	.183 (7.70)	.112 (4.71)	.154 (7.45)
NY Law	.303 (8.52)	.051 (1.76)	.009 (0.04)	.053 (2.55)
Social Controls	No	No	Yes	Yes
Year Fixed Effects	Yes	No	Yes	No
Linear Trends	No	Yes	No	Yes
R-squared	0.962	0.962	0.967	0.969

Note: The dependent variable is estimated seatbelt use, derived by regressing survey data provided by Liran Einav and Alma Cohen on FARS data and using FARS data to predict actual use. Enforcement is calculated as the number of state convictions multiplied by the state's fine, divided by the number of highway vehicle miles in the state (in thousands). Includes data from ME, MN, NH, NM, NY, NC, SD, TN, VA, WI. All regressions contain state fixed effects. Standard errors are clustered at the state level. T-values are in parentheses. Coefficients significant at the 5% level are in bold.

Table 3b: Effect of Cost of State Patrol Tickets per State

	<i>Use (1)</i>	<i>Use (2)</i>	<i>Use (3)</i>	<i>Use (4)</i>
Enforcement	.000135 (1.26)	-.000258 (-2.57)	.000162 (1.30)	-.000163 (-1.43)
Primary Law	.113 (1.94)	.295 (4.32)	.087 (1.49)	.205 (3.28)
Secondary Law	.026 (0.59)	.195 (3.88)	.001 (0.02)	.133 (3.49)
NY Law	.346 (38.20)	.055 (1.88)	.570 (4.94)	.056 (2.04)
Social Controls	No	No	Yes	Yes
Year Fixed Effects	Yes	No	Yes	No
Linear Trends	No	Yes	No	Yes
R-squared	0.968	0.957	0.970	0.969

Note: The dependent variable is estimated seatbelt use, derived by regressing survey data provided by Liran Einav and Alma Cohen on FARS data and using FARS data to predict actual use. All regressions contain state fixed effects. Enforcement is calculated as the number of tickets given out by the state police or highway patrol multiplied by the state's fine, divided by the number of highway vehicle miles in the state (in thousands). Includes data from AL, ID, IL, IN, KY, NH, OH, SC, TX, WY. Standard

errors are clustered at the state level. T-values are in parentheses. Coefficients significant at the 5% level are in bold.

Table 3c: The Impact of Fine Level on Seatbelt Use

	<i>Use (1)</i>	<i>Use (2)</i>	<i>Use (3)</i>	<i>Use (4)</i>
Fine Level	.000626 (1.53)	.000553 (1.23)	-.000302 (-0.46)	-.000096 (-0.13)
Secondary Law	-.086 (-5.44)	-.057 (-0.75)	-.065 (-3.03)	-.069 (-3.72)
Social Controls	No	Yes	No	Yes
Year Fixed Effects	Yes	Yes	No	No
Linear Trends	No	No	Yes	Yes
R-squared	0.866	0.873	0.889	0.895

Note: The dependent variable is estimated seatbelt use, derived by regressing survey data provided by Liran Einav and Alma Cohen on FARS data and using FARS data to predict actual use. All regressions contain state fixed effects. Coefficients for secondary laws are relative to primary laws, not to the absence of a law. Includes data from all fifty states plus the District of Columbia. Standard errors are clustered at the state level. T-values are in parentheses. Coefficients significant at the 5% level are in bold.

Table 3d: Effect of Number of Convictions per State

	<i>Use (1)</i>	<i>Use (2)</i>	<i>Use (3)</i>	<i>Use (4)</i>
Enforcement	.00892 (1.67)	.01111 (3.32)	.00531 (1.09)	.00418 (0.63)
Primary Law	.186 (5.09)	.222 (4.59)	.213 (6.29)	.211 (10.55)
Secondary Law	.080 (2.13)	.170 (7.59)	.107 (4.11)	.150 (6.76)
NY Law	.302 (7.11)	.050 (1.82)	-.073 (-0.33)	.053 (2.58)
Social Controls	No	No	Yes	Yes
Linear Trends	No	Yes	No	Yes
R-squared	0.961	0.963	0.966	0.969

Note: The dependent variable is estimated seatbelt use, derived by regressing survey data provided by Liran Einav and Alma Cohen on FARS data and using FARS data to predict actual use. All regressions contain state fixed effects, and those that do not have linear trends have year fixed effects. Enforcement is calculated as the number of state convictions, divided by the number of highway vehicle miles in the state (in thousands). Includes data from ME, MN, NM, NY, NC, SD, TN, VA, WI. Standard errors are clustered at the state level. T-values are in parentheses. Coefficients significant at the 5% level are in bold.

Table 3e: Effect of Number of Highway Tickets per State

	<i>Use (1)</i>	<i>Use (2)</i>	<i>Use (3)</i>	<i>Use (4)</i>
Enforcement	-.00653 (-0.58)	.00372 (0.36)	-.00900 (-0.90)	-.00166 (-0.14)
Primary Law	.170 (4.62)	.202 (2.77)	.159 (4.42)	.157 (2.74)
Secondary Law	.056 (2.17)	.155 (3.44)	.046 (1.41)	.108 (2.93)
NY Law	.313 (15.13)	.043 (1.41)	.677 (3.72)	.051 (1.80)
Social Controls	No	No	Yes	Yes
Linear Trends	No	Yes	No	Yes
R-squared	0.968	0.955	0.970	0.968

Note: The dependent variable is estimated seatbelt use, derived by regressing survey data provided by Liran Einav and Alma Cohen on FARS data and using FARS data to predict actual use. All regressions contain state fixed effects, and those that do not have linear trends have year fixed effects. Enforcement is calculated as the number of highway patrol citations, divided by the number of highway vehicle miles in the state (in thousands). Includes data from AL, ID, IL, IN, KY, NH, OH, SC, TX, WY. Standard errors are clustered at the state level. T-values are in parentheses. Coefficients significant at the 5% level are in bold.

Table 3f: The Natural Log of Use in States with Conviction Data

	<i>ln(use) (1)</i>	<i>ln(use) (2)</i>	<i>ln(use) (3)</i>	<i>ln(use) (4)</i>
Enforcement	-.000688 (-1.08)	.000066 (0.08)	-.000384 (-0.66)	-.000421 (-0.42)
Primary Law	.775 (3.32)	.771 (3.23)	.632 (3.29)	.571 (2.34)
Secondary Law	.337 (4.71)	.724 (3.33)	.259 (1.97)	.521 (2.38)
NY Law	1.019 (8.72)	1.052 (4.64)	1.004 (1.92)	.921 (3.73)
Social Controls	No	No	Yes	Yes
Linear Trends	No	Yes	No	Yes
R-squared	0.941	0.922	0.946	0.929

Note: The dependent variable is estimated $\ln(\text{seatbelt use})$, taking the natural log of Percent "Known" Restrained in the FARS data. Regressions contain state fixed effects, and those that do not have linear trends have year fixed effects. Includes data from ME, MN, NM, NY, NC, SD, TN, VA, WI. Standard errors are clustered at the state level. T-values are in parentheses. Coefficients significant at the 5% level are in bold.

Table 3g: Use in States That Provided State Police Citation Data, Not Accounting for Enforcement

	<i>Use (1)</i>	<i>Use (2)</i>	<i>Use (3)</i>	<i>Use (4)</i>
Primary Law	.150 (5.14)	.217 (3.08)	.139 (4.71)	.150 (3.10)
Secondary Law	.043 (1.42)	.158 (3.24)	.034 (1.00)	.106 (2.94)
NY Law	.339 (26.98)	.042 (1.36)	.584 (3.96)	.051 (1.96)
Social Controls	No	No	Yes	Yes
Linear Trends	No	Yes	No	Yes
R-squared	0.967	0.954	0.969	0.968

Note: The dependent variable is estimated seatbelt use, derived by regressing survey data provided by Liran Einav and Alma Cohen on FARS data and using FARS data to predict actual use. All regressions contain state fixed effects, and those that do not have linear trends have year fixed effects. Includes data from AL, ID, IL, IN, KY, NH, OH, SC, TX, WY. Standard errors are clustered at the state level. T-values are in parentheses. Coefficients significant at the 5% level are in bold.

Table 4: Effects of the New York Law

	<i>NY</i>	<i>Near (1)</i>	<i>Near (2)</i>	<i>Far (1)</i>	<i>Far (2)</i>
NY Law	.504 (12.63)	.108 (5.15)	.063 (0.13)	.091 (2.99)	.254 (0.55)
Primary Law		.090 (4.09)	.081 (1.91)	.353 (15.42)	.218 (8.2.80)
Secondary Law		.138 (7.63)	.131 (3.30)	.131 (3.59)	.082 (0.85)
Controls	No	Yes	Yes	Yes	Yes
Linear Trends	No	No	No	No	No
State & Year Fixed Effects	No	No	Yes	No	Yes
R-squared	0.832	0.940	0.973	0.959	0.990

Note: The dependent variable is estimated seatbelt use in either "near" states or "far" states, derived by regressing survey data provided by Liran Einav and Alma Cohen on FARS data and using FARS data to predict actual use. "Near" states are CT, NJ, PA, and VT; "far" states are CA, OR and WA. State use is not weighted by population or drivers; each state is weighted equally. Standard errors are clustered at the state level. T-values are in parentheses. Coefficients significant at the 5% level are in bold.

Table 5: The Effect of Tennessee's Law Without Sanction

	<i>Use (1)</i>	<i>Use (2)</i>
Law Without Fine	.191 (7.57)	.326 (10.20)
Law with Fine	.373 (17.02)	.479 (18.41)
Secondary Law		-.135 (-5.17)
R-squared	0.900	0.946

Note: The dependent variable is estimated seatbelt use in Tennessee, derived by regressing survey data provided by Liran Einav and Alma Cohen on FARS data and using FARS data to predict actual use. Coefficients are relative to a primary law. T-values are in parentheses. Coefficients significant at the 5% level are in bold.

Table 6a: Perceived Enforcement Where Observations Are Individuals

	<i>Use (1)</i>	<i>Use (2)</i>	<i>Use (3)</i>	<i>Use (4)</i>
Primary Law	.381 (3.59)	.338 (3.15)	.215 (1.71)	.253 (2.00)
Secondary Law	.272 (2.56)	.251 (2.33)	.208 (1.83)	.190 (1.44)
Perceived Enforcement		.053 (6.64)	.041 (2.22)	.087 (4.63)
Enforcement (Convictions)			.021 (2.48)	
Enforcement (Citations)				-.00145 (-0.15)
R-squared	0.009	0.019	0.027	0.030

Note: Based on the Motor Vehicle Occupant Safety Survey 2007 data set, which used a nationally representative sample of individuals. The dependent variable is a combined measure of self-reported frequency of use of shoulder and lap belts. Enforcement (convictions) is calculated as the number of state convictions, divided by the number of highway vehicle miles in the state (in thousands); Enforcement (citations) is calculated as the number of highway patrol citations, divided by the number of highway vehicle miles in the state (in thousands). The first and second regressions contain data from all fifty states. The third column contains data from ME, MN, NM, NY, NC, SD, TN, VA, WI. The fourth column contains data from AL, ID, IL, IN, KY, NH, OH, SC, TX, WY. T-values are in parentheses. Coefficients significant at the 5% level are in bold.

**Table 6b: Perceived Enforcement Where Observations Are States
(2007 only)**

	<i>Use (1)</i>	<i>Use (2)</i>	<i>Use (3)</i>	<i>Use (4)</i>	<i>Use (5)</i>	<i>Use (6)</i>
Primary Law	.250 (2.22)	.174 (1.31)	.128 (0.87)	-.486 (-2.35)	.333 (6.10)	.257 (1.35)
Secondary Law	.131 (1.16)	.083 (0.68)	.047 (0.36)	-.248 (-2.06)	.250 (4.57)	.193 (1.32)
Perceived Enforcement		.074 (1.08)		.475 (3.30)		.059 (0.42)
Enforcement (Convictions)			.021 (1.35)	.035 (3.31)		
Enforcement (Citations)					-.031 (-5.19)	-.028 (2.90)
R-squared	0.268	0.285	0.269	0.724	0.818	0.790

Note: The dependent variable is estimated seatbelt use, derived by regressing survey data provided by Liran Einav and Alma Cohen on FARS data and using FARS data to predict actual use. Enforcement (convictions) is calculated as the number of state convictions, divided by the number of highway vehicle miles in the state (in thousands); Enforcement (citations) is calculated as the number of highway patrol citations, divided by the number of highway vehicle miles in the state (in thousands). The first and second columns contain observations for all fifty states. The third and fourth columns contain observations for ME, MN, NM, NY, NC, SD, TN, VA, WI. The fifth and sixth columns contains observations for AL, ID, IL, IN, KY, NH, OH, SC, TX, WY. These last four columns are based on only ten observations each. T-values are in parentheses. Coefficients significant at the 5% level are in bold.

Table 6c: Impact of High-Visibility Click It or Ticket Campaigns

	<i>Use (1)</i>	<i>Use (2)</i>	<i>Use (3)</i>	<i>Use (4)</i>
CIOT	.016 (0.43)	.009 (0.30)	.026 (0.73)	.019 (0.69)
Primary Law	.190 (5.09)	.227 (4.45)	.217 (6.59)	.215 (10.82)
Secondary Law	.081 (2.03)	.172 (7.03)	.108 (4.02)	.150 (6.70)
NY Law	.304 (2.03)	.052 (1.85)	-.144 (-1.25)	.056 (2.60)
Enforcement	.00774 (1.44)	.0115 (2.88)	.00378 (0.74)	.00508 (0.68)
Social Controls	No	No	Yes	Yes
Linear Trends	No	Yes	No	Yes
R-squared	.962	.963	.967	.970

Notes: The dependent variable is estimated seatbelt use, derived by regressing survey data provided by Liran Einav and Alma Cohen on FARS data and using FARS data to predict actual use. All regressions contain state fixed effects, and those that do not have linear trends have year fixed effects. Enforcement is calculated as the number of state convictions, divided by the number of highway vehicle miles in the

state (in thousands). Regressions contain data from ME, MN, NM, NY, NC, SD, TN, VA, WI. CIOT takes the value of 1 in a year the state had a *Click It or Ticket* campaign. Standard errors are clustered at the state level. T-values are in parentheses. Coefficients significant at the 5% level are in bold.

Table 6d: Effect of Number of Convictions per State, Lagged One Year

	<i>Use (1)</i>	<i>Use (2)</i>	<i>Use (3)</i>	<i>Use (4)</i>
Enforcement (lagged)	.00911 (1.77)	.01437 (2.01)	.00608 (1.38)	.00602 (0.61)
Primary law	.180 (5.37)	.179 (4.12)	.192 (6.32)	.171 (3.17)
Secondary law	.077 (2.39)	.138 (4.41)	.096 (3.78)	.123 (3.36)
NY law	.254 (8.44)	.045 (1.56)	.017 (0.09)	.038 (1.84)
Social controls	No	No	Yes	Yes
Linear trends	No	Yes	No	Yes
R-squared	.958	.960	.963	.967

Notes: The dependent variable is estimated seatbelt use, derived by regressing survey data provided by Liran Einav and Alma Cohen on FARS data and using FARS data to predict actual use. All regressions contain state fixed effects, and those that do not have linear trends have year fixed effects. Enforcement is calculated as the number of state convictions, divided by the number of highway vehicle miles in the state (in thousands), and it is lagged one year, so use in year t is a function of enforcement in year $t-1$. Includes data from ME, MN, NM, NY, NC, SD, TN, VA, WI. Standard errors are clustered at the state level. T-values are in parentheses. Coefficients significant at the 5% level are in bold.

